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THE INFLUENCE OF VISUAL ERRORS IN MUSKETRY.

By MR. J. H. SUTCLIFFE.

Discussion.—25th November, 1915.

PROFESSOR EDSER said he had listened with great pleasure to the address given, and he was particularly glad to hear that at last the Government were taking steps to act in a sensible manner. It was ridiculous that a man should be incapacitated from a great public service merely because of a small defect in the eye which could be corrected by the application of known principles. It had in past times very often been said—he believed quite erroneously—that in Germany and France eyesight was worse because more of the people in those countries wore spectacles than was the case in England. Whether that was true or not, the question of whether defects in eyesight were corrected or not naturally depended to a large extent on the necessity for such correction. With field labourers and such-like there was no particular necessity for correction, but when people had to do work requiring accurate eyesight, then of course the necessary correction must be applied, and in recent times many more people had come to wear spectacles in England than used to be the case when he was a boy. At that time people chiefly wore spectacles when they reached an age at which they began to suffer from presbyopia; otherwise they mostly tried to get on without glasses, whatever defects they might have. Nowadays a larger proportion of the people of this country were engaged in work requiring greater visual acuity, and therefore they had to wear glasses.

With regard to the aiming device mentioned by Mr. Sutcliffe, he would like, as its inventor, to say a few words in order to explain its exact object. It was designed to train people in the quickest possible manner in the preliminary part of the art of rifle-shooting. He did not mean to say that with the aid of that device a man was going to win a prize at Bisley, but he claimed that by its use it was possible to effect an enormous economy of the time usually occupied in getting over the initial errors of people learning to shoot. It was also extremely instructive to the sergeant who had to look after the men who were learning to shoot. The idea that this preliminary training was required was not a new one. In the old Army they had devices for doing all the things done by his apparatus, but they were extremely difficult to use, and very often were for that reason rendered of no value. There were two things necessary in shooting. First of all, the barrel of the rifle must be put in the right position, and, secondly, it must be properly sighted on to the object. He need not go into the question of the adjustment of the sights; he would suppose them to be properly adjusted. In sighting the rifle, of course, one required to have fairly good sight, but it was a most extraordinary thing how much a person's visual acuity would increase with practice. A friend of his at the Goldsmiths' College last summer, when the war broke out, could not, with his spectacles or without them, see the bull's-eye on the target at all. After three months' training, he could see not only the bull's-eye, but the marks of his shots on the target. Now, during that time his sight had not improved in the slightest degree—he was a man over thirty years of age, a time of life when one did not expect improvement. It was merely an illustration of a point mentioned by Mr. Sutcliffe, the mental training, the training of seeing a thing and also interpreting what was seen.

It was sometimes supposed that the eye, optically speaking, was rather a perfect optical instrument. That was quite wrong. Optically speaking, the eye would be a very poor instrument, but the great beauty of the eye, and that which rendered it one of the most marvellous things they knew of, was that with practice, with continually using it, its owner could not readily see what the faults of the eye were, and automatically discounted all the errors that occurred. That, no doubt, was the origin of what Mr. Sutcliffe had said about the Boers, that they were used to looking at things a long way off on the veldt, and so could distinguish things which we could not. He had noticed in his younger days when yachting that a sailor would be able to pick out a buoy among all the glitter of the waves while a person not used to the conditions could see nothing except the continual glitter of the water. Then, supposing a man to possess the necessary spectacles, and also to have received the necessary training in seeing things, there was still something else to be done in teaching him to shoot. He must hold the rifle steady while he pressed the trigger. It was generally called "pulling" the trigger, but to "pull" the trigger was wrong. It almost certainly resulted in the rifle being twisted round. If one told that to a young man beginning to learn to shoot, he was apt to think one a fool, because it seemed so obvious a thing. He would therefore aim at the target, pull his trigger, and shoot wide, and then he would immediately think there was some error in the sights of the rifle, because this was something he was not accustomed to at all. He had trained between 100 and 200 men belonging to Volunteer corps with the device in question, and had found this interesting thing—that nearly every one of these men, unless they had great defects in their eyes, if provided with ordinary spectacles, aimed pretty well. They directed the rifle quite correctly

when they began, but they were not quite satisfied with that in the first instance, and would think they had not done quite well enough, and that by waiting they would do better in time. Then they began wavering, the eyes began to get tired first of all, and the muscles got tired, and the consequence was their aim got worse and worse; you could see them wandering round and round, getting worse and worse, until at last, when they pulled the trigger, they pulled the rifle round in the way he had described.

Miniature Rifle Shooting.

The remedy which had been applied to that was shooting at a miniature rifle range. But unfortunately a very large proportion of the miniature rifles did not shoot straight; the bores were very small, the bullets were very small lead bullets and not very well made; and quite a large proportion, he should say fifty per cent., of the rifles at the miniature rifle ranges would not shoot straight, and it was therefore no good practising with them. The man looking after the range would very often say, when a man missed, and his bullet went too high, "Aim lower." Now, that was exactly what one ought not to do; one ought always to aim exactly at the bull's-eye. If the sights were wrong they ought to be corrected, and if a shot went too high the reason of that ought to be ascertained and the fault remedied. Well, that was the object of his device—first of all, to see whether the man was aiming correctly, because an expert first of all aimed, and someone marked on the target the position of the little line of light, or some point of that line of light. An expert did that first of all, and therefore anyone standing by the target could say at once whether the learner was aiming correctly or not, and if he "pulled" the rifle round in the way already described, what he did could be immediately

seen and corrected. He had found that plan work out extremely well. One man had informed him that he had never fired with any kind of gun in his life, and after spending about two and a half hours with this device he went into a miniature rifle range with a good rifle he had purchased, and the very first time he shot he scored 75, while in the ordinary way a man starting would make 20, 30, or perhaps 40 if he happened to be particularly steady-handed. In the instance in question the man in charge of the range would not believe that this individual had never actually fired before.

Optical Training for Marksmen.

It seemed to him (Professor Edser) that it would be a very useful thing to employ this device having regard to the necessities of the present time in the way of providing facilities for training the Army. With the very large number of men they now had to deal with it was almost impossible to get a sufficient number of ranges to give the necessary preliminary training, because certain of the things he had been mentioning were errors which had to be got over before a man could hope to be anything like a marksman. When those were once got over a man could then go to the long range and begin to overcome the other difficulties which still confronted him. His device possessed the advantage that it required no ammunition, and therefore there was no danger attached to it. It could be utilised in any room which was sufficiently dark. The best distance was about ten yards. That gave full play to the use of the eye, and a skilled man was not required except in the first instance for aiming the rifle and finding the position where the little point of light should be. After that it was sufficient to have two men, both learning, and let one stand near the target and mark the position of the other man's shots for him, telling him the things he was doing wrongly;

then presently let them interchange, the other doing the same for the first man. In that way the man observing the target saw exactly what his friend was doing, and was therefore not surprised when he in his turn was told that he was doing things wrong. He had hoped that his device would have been taken up by the Army, but unfortunately the Army authorities did not require, or said they did not require, anything of the sort at all. This seemed to him unfortunate, because under existing conditions a great amount of time and a large number of skilled men were necessary to the work of training our enormous armies of recruits, and he could not help thinking that we had neither the time nor the men necessary to properly carry out this work. (Applause.)

Frequency of Anisometropia.

DR. CLAY said there were in Mr. Sutcliffe's paper two main divisions, one the effect of visual errors on shooting, and the other the effect of errors in looking through telescopes and binoculars. He would deal with the second one first. He had been looking at a prism binocular that morning and had noticed something which he believed was common to prism binoculars. This had an adjustment on one of the eye-pieces by which it was possible to focus that eye-piece independently of the other one, so that a person who had unequal sight could focus one eye with the main screw and then set the adjustment for the other eye. And he had said to a gentleman in the room that he did not know what the statistics were, but that his impression was that the number of people to whom that would be of any use was exceedingly small, probably of the order of two or three per cent. That would probably be about the proportion of people to whom it really mattered that they should be able to change the spherical power of the one eye as compared with the other. But it did seem to

him that if it were possible to supply what Mr. Sutcliffe had suggested, some little lenses which one could put on to one's own particular prism binoculars, it would be a good thing. One could always correct equal spherical errors for the two eyes by merely focusing; the only thing which could not really be corrected by focusing was the astigmatic error, and the proportion of people who had a slight astigmatic error was very large; as Mr. Sutcliffe had said, something like fifty per cent. would probably be included if one went down as far as .25. That was to say that one out of two persons using binoculars would be able to see very much better if they were able to add their own astigmatic correction. And it would have another advantage which was perhaps worth mentioning, namely, that no one else could borrow one's glasses and carry them off—(laughter)—they would be of very little use to that person unless his astigmatic error was exactly the same as that of the owner of the glasses.

Importance of Low Astigmatism.

Anyone who suffered, as he himself did, from a small amount of astigmatism knew what an enormous difference that small error made when one was trying to use a telescope or a binocular to the best advantage. If one was looking at something one really required to see, of course one fixed the telescope or binocular independently of one's self, resting it on a wall or something of that kind and balancing it so that one did not have to touch it, and then looking through it, and if one had, say (as he had) .7 error, a great difference was found in the amount of definition obtained. And it would be of very great value in searching, say, the German trenches for small holes and that kind of thing to have this little correction added to the telescope or binocular. Of course, the difficulty was that they would have to supply that correction to everybody for his own particular

glasses. Still, if one were buying a pair of glasses for oneself, a matter of a few shillings extra would not matter very much if one felt sure that as a result of that extra expenditure the instrument could be used to very much greater advantage. So that he really felt it was a point which was not at present paid sufficient attention to. Most of those present were no doubt sufficiently accustomed to looking through telescopes and high-power binoculars to appreciate the enormous difference it made if one was able to fix those instruments and not have to hold them. He generally amused himself at the seaside by taking his telescope and watching the shipping or anything that was going on, and he had noticed over and over again—on his last holiday in particular—that if he wanted to see something very particularly on board, say, a vessel at some distance out, that it made a simply tremendous difference if he could support the telescope independently. He used to put his telescope in the camp chair, turn the chair round so that the telescope rested in the angle of the chair, and move the sand about until the instrument pointed in the right direction, then lie down behind it and get a view that was, without exaggeration, three or four times as good as he could obtain if he had to support one end and look through the other. Because if one got a magnification of 10 or 20, and most telescopes had a much higher magnification than that, and one end of the telescope had to be held, the motion was magnified ten or twenty times; so that the actual quiver produced by one's own unsteadiness was very marked indeed.

Choice of Miniature Rifle.

Then turning to the other question, which was also a very interesting one—the question of the sighting of a rifle—his experience of miniature rifles was not as unsatisfactory as that of Professor Edser. They had been running a

range at the Polytechnic, and from the beginning he had taken a very great deal of interest in the instruction in firing. They had a dozen miniature rifle ranges, and found that when a man really could shoot they made very good targets. One man made 100 out of 100, and a great many had made 97 and 98, so that it was not the fault, in their case at any rate, of the miniature rifles if they failed to get good scores. Some long time ago they had had Morris tubes fitted, but had found them very unsatisfactory. The tube seemed to be displaced, and if screwed up a little tight it worked loose, and finally got locked. But with a solid-bore small miniature rifle that did not occur, and it was possible to get really good shooting. Of course, the sighting was not always perfect, but they used to let the instructor shoot at first and set the sights, and that got rid of the difficulty. He had been very much interested in what Mr. Sutcliffe had said, and in Professor Edser's explanation, as to what occurred in aiming. For his own part the way he aimed, and the way he had instructed men to aim, was to first of all fix attention on the sights of the rifle, and let the target more or less take care of itself. Then, fixing attention on the fore sight alone, one could see the back sight quite well enough to get the fore sight in the middle of it—to get the eye, the back sight, and the fore sight in a line. That took very little time, and then, moving as a whole, one could see the target in an indistinct way, so that that was more or less also in a line, and then, keeping the gun and oneself rigid, one could move about quite a lot, and the eye, back sight and fore sight would remain in line. Then attention was fixed on the target, and then the fore sight could be seen sufficiently well. Then, the fore sight being got on to the target, all four were in line, the trigger was pulled, and the deed was done. Of course, that was all right if one had time, and was shooting carefully, but if shooting in

a hurry, then he thought that in all probability what happened was what Mr. Sutcliffe had said, that the target was seen clearly, the fore sight somewhat indistinctly, and the rear sight still more indistinctly, and by practice it was possible to get that "haze" in the right place and to, as it were, feel that the things were in a line.

A Popular Error.

He (or they) did not believe in the theory that the eye accommodated backwards and forwards 200 times a second; that did not appeal to him at all, and he did not think that sort of thing could be done unconsciously. Of course, he quite believed one could change one's accommodation very suddenly; one thought of the fore sight and of the target, and almost in the moment of the thought one's accommodation changed automatically. He thought they were all indebted to Professor Edser for showing them his method of pointing the gun and of checking the error in pulling. With beginners no doubt that was the greatest difficulty, and as Professor Edser had said, beginners did not believe that they pulled, and were not at all convinced their own legs were not being pulled when told that they had done so. They did not feel that anything happened; they did not see that the rifle shifted, and they did not believe that they would do much better if they did not pull. And anything that would convince them that they were making that mistake and therefore must correct that mistake, was well worth doing. Of course, there were other ways which were sometimes employed for doing that—by locking the sights, and so on.

With regard to the use of glasses in shooting, under present conditions, in trench warfare, where the man was, of course, not lying down on the ground, and was not looking along at a great angle, ordinary spectacles were quite all right. Standing up, and looking along the

gun, he could see quite well through ordinary glasses, but when lying down on the ground, as a man would be if in advancing against the enemy he fell down and wanted to shoot someone in the distance, then he would be looking almost through his eyebrows, and an ordinary pair of glasses would not be of the slightest use, because the man would be looking right above them. So that if persons with very bad sight were to be in the firing line something very special in the way of glasses would be required.

MR. BAKER was sorry he could not agree with what Mr. Sutcliffe had said with regard to the use of spectacles for looking through telescopes and binoculars. One could not look through the cup-shaped eye-piece of a telescope with an ordinary pair of spectacles at all, and in his opinion one would be much worse off as regards getting the value of a pair of binoculars by looking through them with the eye about half-an-inch behind the Ramsden circle than by accommodating as far as possible with the focusing arrangement of the binoculars and simply doing without any cylindrical correction at all.

Advantage of Best Form Lenses.

MR. P. F. EVERITT thought there was no reason why, in the case of binoculars, the Ramsden circle should not be put half an inch behind instead of only a quarter of an inch. It was only a question of a suitably designed instrument. And the same with the telescope. It was much better always that the glasses should be worn by the observer than to try and fit them to the instrument. With the telescope there was no means whatever of making sure the axis of cylinder was in the right direction unless there was a special indicator on the telescope, or something of that sort, which would be most inconvenient. As regarded Dr. Clay's suggestion of putting an instrument on the ordinary lens to suit a case of oblique vision, there would be no difficulty about that at all. Mr. Sutcliffe

appeared to be doubtful whether, in shooting, the spherocylinder should be flat, or whether, as he thought, the meniscus form would be better. He had done a good deal of shooting during the past five or six years with good glasses, and had found that in the case of a person requiring minus spectacles the additional power obtained by looking through the edge, as one had to do, was a distinct advantage. Among shooting men it was generally recognised that anyone with a tendency towards long-sightedness had an advantage over a person with perfectly normal vision.

Mr. F. W. DADD said he had been rather interested in rifle shooting, especially since the war, and he congratulated Professor Edser on his attachment to the rifle. Many had to shoot out in the open; he himself had been doing a great deal of shooting at their electricity works, and on a cold afternoon, with all the smoke blowing down, it was not very pleasant. But an attachment like Professor Edser's could be used in a room, and all the training done by night, which was a consideration of very great value just now. In his opinion it was a very fine arrangement. From his observation of men learning to shoot they did generally, as the Professor had said, pull the trigger instead of pressing it. With regard to shooting spectacles, he had fitted a great number of men, and he thought the double-action joint quite answered the purpose. A man lying down shooting was able to tilt the lenses outwards, and seemed to get quite a good result. That was his experience. In shooting it was a question of practice; a man did not really accommodate, as they understood the term, he really ignored his back sight, and seemed to get the feeling that he had the bull and sights in line. He thought that feeling really came with practice. The usual advice given in Army shooting was that you should not aim at the bull at first, but get towards the ring of the target and aim

for "6 o'clock," gradually come along to it, and, having found "6 o'clock," press the trigger. But so many men would aim at the bull, and that was where they went wrong. At a certain range at Brixton a few months ago a very well known shot was explaining to the recruits there that that was the method he advised, and he had noticed afterwards an astonishing improvement in the shooting of half-a-dozen men. He hoped that Professor Edser would bring his arrangement to the notice of the Volunteer Training Corps Association, to whom he thought it should be of the greatest value.

Shooting Spectacles.

MR. H. A. HUGHES said he had often come across cases of veteran seamen who, when tried with the distance test card, would assert that they had most magnificent vision, but they were invariably found to be hypermetropes, and if it was suggested that they wore glasses for distance, they replied they did not require them at all. He had fitted one or two of those double-action joints for shooting, and they were certainly an advantage.

MR. BROWNE asked for an explanation of a discrepancy. Mr. Fleming had said he was taught to shoot at "6 o'clock," whereas Professor Edser had said one ought to shoot straight at the bull's-eye.

PROFESSOR EDSER replied that he was referring merely to the point one ought to aim at; he did not mean that one should aim at the middle of the bull's-eye. He always aimed just in the way shown on the diagram.

MR. BROWNE said in that case he did not quite understand what "aiming" was. He himself had been taught to aim at "6 o'clock," and he understood that what happened was that one could not thoroughly correct one's error in pulling, and that that to a certain extent

allowed the correction of an error that one could not get rid of.

MR. DADD thought that, as far as the "6 o'clock" aim was concerned, the experience at Bisley was that they had to change their aim according to the conditions very frequently. He had known very good shots aim at the top of the target, then at the bottom left-hand corner, and so on, whatever suited the light best. It was a bad habit to get into one fixed method of shooting; men should adapt themselves to whatever they could see best, and make allowances on the rifle accordingly.

PROFESSOR EDSEER said if a man merely went on aiming at different parts of the target until he hit the bull's-eye he did not know how much of the divergency was due to error of rifle or error of pulling, and if he got another rifle and another target he was no better than when he started. If a man had had so much practice that he knew that he could see one thing better than another, and aimed that way, of course it was all the better.

Changes of Light.

MR. MAURICE BLOOD thought it was pretty generally recognised that to aim at the bull was very dangerous. In competition shooting it was obviously very desirable if possible to get such an aim as to have a constant elevation and direction under all conditions. Of course, the elevation was usually more troublesome in any changes of light, such as the evening sun setting behind the target, or a dark thunder-cloud coming up, or a certain amount of mist. If one was aiming under the bull it was almost certainly desirable that it be some sensible distance under. Naturally, there were difficulties in keeping that space exactly the same. In bright light one was apt to go closer to the bull, and a further refinement was whether there was more light on the target or

on the sight, and so on. There were very many devices used other than aiming underneath the bull. One which was often very successful was just covering the bull with the foresight. This aim was always used at 600 yards by the man who was undoubtedly the finest service rifle shot of recent years, who had unfortunately been recently killed in action.

Possibly the most important thing he could contribute to the discussion was as to the question of aiming—whether one ought to try and accommodate from one to the other, back sight, fore sight, and bull's-eye, or to concentrate the focus on one only. It sounded rather a curious thing to say, but he believed he had put two men into the way of winning the Service Rifle Championship at Bisley by advising them to focus the fore sight only. Endeavouring to compensate from one to the other, he personally thought, was quite hopeless with the great majority of men. He used to do it years ago with one eye—not the eye he shot with now. At the present time he could not do it with either eye. His impression was very strongly that it was desirable with the great majority of people to focus on the fore sight, and that there was a great danger of allowing the focus to wander, especially towards the end of a shoot. One of the two men he had mentioned, who had also lost his life in Flanders, and who was known to be an extremely good holder, came to him in great trouble. He would get a low right shot towards the end of a shoot, and on looking through his score book he found it was nearly always the fifth or sixth shot. The speaker suspected that the eye was getting tired. The shooter was not really quite sure what he focused on. After shooting again he said he focused on the fore sight. He (the speaker) recommended him to keep on the fore sight all the time, and not to let the focus wander, with the result that the trouble was absolutely eliminated.

Accommodation and Axis Movement.

The reason why he thought it so very desirable to keep on one, and not to vary from one to the other, was that a great many people had a curious defect—that the line of aim was not the same if they altered the focus. If the rifle was put in a fixed rest aiming at the target, when they focused the bull's-eye and focused the front sight, they found the aim was distinctly different. One speaker had said that a short-sighted person was at a disadvantage. He disagreed with that view. He thought a person with a slight amount of myopia had an advantage for shooting. The last winner of the Service Rifle Championship was myopic, and on his (the speaker's) advice he got glasses not sufficiently strong to clear the target, so that he could only focus on the fore sight; he won easily.

MR. R. J. FLEMING said that as to a slight degree of myopia being an advantage it depended on the sort of sight the man was using. At Bisley it did not matter, but with open sights it was necessary to correct it.

[The aiming instrument designed by Prof. Edser consists essentially of an electric torch having a 4 D. projecting lens and forming an attachment to the rifle, so that while the recruit cannot see whether he jerks the rifle when firing, his instructor can note at the moment of pressing the trigger the position on the target of the projected spot of light.—ED.]

IMPROVEMENTS IN PRISMATIC COM-
PASSES WITH SPECIAL REFERENCE
TO THE CREAGH-OSBORNE
PATENT COMPASS

By MR. HENRY A. HUGHES.

Read 9th December, 1915.

THE compass is primarily an instrument of navigation—the navigator's best friend. With it, during several days of fog without visible star or other means of checking his position, he makes his course, and the fog lifts to show him within sight of his bearing. Yet all instruments of navigation, however sensitive and accurate they may be, are subject to large errors arising from the circumstances and position in which they are used.

The sextant, for example, is generally verified at the N.P.L. to 10 seconds in its readings, but error due to refraction in the horizon amounts to minutes of arc.

So also the ship's compass is subject to errors arising from vibration, heeling, and friction of several degrees, though certified correct on all points.

The gradual elimination of these errors has been the result of laborious experiment and much thought, and has resulted in the laying down of general principles and the construction of magnetic compasses, which are the most accurate and sensitive instruments that have been made. The compass, which is of such primal importance to the navigator, has, however, owing to recent developments in aircraft, become of equal importance in military use.

Armies are forced more and more to fight under cover of darkness. Aeroplanes, which at present are able to report every movement of the enemy, may before long overcome the difficulty of landing in the dark, but they will never be able to carry out any valuable reconnaissance at night.

"The army best equipped for night fighting would be victorious in a European war," said a well-known authority, and the ability to find the way has an influence on the fighting efficiency. Fog could possibly be used to advantage in conjunction with the very perfect maps available.

A grandson of Archibald Smith, who did such valuable work for the Compass Department of the Admiralty, inspired by the records of his ancestor, has devoted himself to compass work with great enthusiasm. He impressed me as being as able to find his way with equal confidence at night as in the day. He used one of the compasses shown here. He could grip his compass to his eye like a monocle.

There is also a dry compass made with a clinometer, and over a year ago an officer devoted careful attention to it, and introduced it as a result of his tests, and a large number are being used to-day with great advantage. I have had many letters from the front to supply a compass exactly as used by this officer and fitted with a radium luminous card.

There are two types of compasses: (1) dry compasses, like the present Mark VII. service pattern; (2) liquid compasses. But in both the requirements are the same, and may be summed up in the following Admiralty notes on the efficient points of a compass.

The essential qualities of a good compass may be considered to embrace great sensitiveness and steadiness with simplicity of construction. By sensitiveness and steadiness it is to be understood that the needle is freely to submit to the earth's magnetic force, with power sufficient to steadily obey that force under the varying

motions of a ship, without the aid of friction or mechanical implement, steadiness, or rather sluggishness, produced by the latter causes being obtained at the expense of accuracy. The chief points to be attended to in construction are:—

1. Great directive power of the needle with little weight, and consequently little friction on the point of suspension.
2. Permanency of the magnetic power of the needle.
3. Accurate adjustment of the several parts of the compass. This comprises the magnetic axis of the needle coinciding with the north and south points of the card. The accurate centring of the point of suspension within the bowl; the perfect impression of the card.
4. The sight to be arranged so as to turn freely in azimuth without moving the compass bowl or causing disturbance to the card. It should be attached to a graduated scale so as to show the angle between the lubber line and any object as measured on the horizon.

The compass referred to in these notes was the (late) Admiralty standard compass introduced into H.M. Navy in 1842, and which is now used as a landing compass. With it reliable observations of the variation on land can be obtained. The graduated circle is divided to $\frac{1}{2}$ degrees and fitted with a vernier which reads to minutes. This compass when properly adjusted can be said to be the most accurate compass for shore work in existence, but unfortunately its size and weight prevent its more general use on land.

Dry Compasses.

The Mark VII. Vernier pattern compass adopted by the War Office is the best known example of the dry card prismatic compass, and is an extremely accurate and useful instrument, provided it can be used properly. It has such a tendency to oscillate that, although tested to $\frac{1}{2}^{\circ}$, it would only be correct if used on a stand

or plane table; the tremors of the hand are quite sufficient to set up a slight oscillation of 2 or 3 degrees, and the nature of its oscillations prevents the mean being taken. Further, its accuracy must suffer in actual use from frictional error, as the dropping of the card on the pivot will sooner or later produce a flat or dubbed point, but by use of a hard iridium point this might be largely obviated. Its great advantage is the reading of the card from the back by prism in such a position that divisions are brought into the plane of the object observed; but it might be improved in this direction if the prism were made to turn independently of the compass, as the movement of the bowl about the card tends to disturb the needle when taking two or three bearings.

For night marching a complicated system is employed, which can be simplified by using the lubber line—or *ligne de for*, as the French call it—for the actual marching point. This can only be done if the whole of the compass card is illuminated.

The Liquid Type of Compass.

This type of compass, because of its greater steadiness, has been in greatly increasing demand since the commencement of the war.

Navigators have found that, however sensitive a compass is, it must first of all be steady and capable of giving a quick reading. The liquid compass is now used in nearly all ships and aeroplanes, and the unsteady conditions at sea are very similar to the use of a compass in the hand. The action of the needles is damped by floating the card in liquid, and the following are the principal points in construction:—

1. Greater directive power of the needles. Two at least or four give excellent results. But this must not increase the weight of the card, so a float is used.
2. Absence of error due to drag of the liquor or accelerating movements of the pivot by turn-

ing quickly. This is best obtained by use of an inverted pivot resting in a sapphire cup supported from the bottom of the compass bowl, and by making the latter sufficiently large to allow of a cushion of liquor round the card.

3. Accurate adjustment of card, coincidence of magnetic axes, accurate divisions of card.

4. Movement of the prism or sighting apparatus round the verge of the compass. This is important, as moving the card of a liquor compass sets up oscillations which take time to settle.

5. Hard iridium pivots.

6. The card and lubber line should be luminous at night. The use of the lubber line is the simplest and most accurate point from which to determine direction. It corresponds to the movable zero on a scale and is free from parallax.

With a view to obtaining these results a compass was made on the designs of Captain Creagh-Osborne, R.N., Superintendent of the Admiralty Compass Department, and has proved of remarkable value in the present war.

The card is mounted on a float with two powerful needles and an inverted pivot with all divisions and markings clearly outlined in radium compound. A movable prism ring moves round the outside verge of bowl, which is made large enough to allow free movement of card. The lubber line consists of an arrow coated with radium compound, and is continued on the prism ring by means of a hinged pointer for marching purposes or map work. It can be made with or without the setting ring, which is hardly necessary, owing to all the points and figures of the card being clearly visible night and day. To take a bearing with the compass:

TAKING A BEARING.

1. Place the prism in position so that it forms an enlarged image of the divisions and figuring on the card.

2. Set the prism parallel to the lubber line.
3. Hold the compass by the thumb ring with the prism about 6 inches from the eye, and set the V-shaped sight on top of the prism direct on the required mark. This sight is radium painted for night use.
4. Place the eye so that the lubber line is coincident with the sight. These correspond to the fore and back sight, and it only remains to read off the division on the card cut by the lubber line. These divisions are 2° , but as the line is exactly the width between 2, the odd degree is read when the line fills the space or the $30'$ when it half does.

To march on a bearing (1) extend the pointer which is radium painted, and either set it in line with the lubber line, or else in line with setting index of the brass recording scale.

Or (2) sight into the prism, which is clearer owing to the magnified divisions. The brass recording ring enables the compass to be used as a director.

Hitherto we have only considered a prism as a means of sighting, but considerable accuracy can also be got with what is known in artillery as the ghost sight. In the marching or infantry type of Creagh-Osborne compass (so called because it is not suitable for plane table use) a lens and point sight is used. First erect the folding sight and set it at zero on the brass scale. Then place the lens focused on the compass card but nearly vertical and as close up to the eye as possible—like a monocle. The top segment of the lens is out away, and the object is sighted through the aperture. At the same time a magnified image of the sight is seen through the top edge of the lens.

This method of sighting is helped by a physiological point, which, as Dr. Ettles says, is seldom used, namely, that an object such as the sight when seen by one eye when both eyes are kept open is apparently projected into space and occupies the field of the other eye. This

method of sighting is made possible by a steady card. The compass is smaller than the prism compass.

GRADUATION.

The divisions on the card are clearest when 5° , but readings can be accurately taken to 1° . This is the type designed by the Admiralty for armoured cars. When made in a large size excellent results are got with it as a marching compass.

Marching on Salisbury Plain on a recent night march an inexperienced officer told me, after two hours' across country he led his men right up to the door of the mill they were marching on. Officers can take a 4-point march and come back on to their position. It can also be made in a simplified form with a fixed ring, and then costs less to make than the Service Mark VII. compass.

A further improvement in marching compasses on the same lines has recently been brought out. This consists of a card mounted on a float and needles and inverted pivot with a vertical scale, and enclosed in a large cylindrical bowl with a small window at the back of the cylinder. On the top of the cylinder is mounted the usual divided ring with revolving prism. The compass can be set up with a staff or plane table in a given direction, and then bearings can be taken all round without moving the bowl.

This compass is also extremely useful for night marching, as all the points, figures, and divisions on the vertical card are in radium compound, and a quick and reliable course is obtained night or day without reference to any line of direction.

A very important feature of these compasses is the radium painted cards. The old Balmain's luminous paint used so much for compasses during the last twenty years requires exposure to light to render it luminous, but in the tests at

the N.P.L. for radium cards they are kept four hours in the dark before the measurement of their luminosity is made.

The introduction of radium painted cards into compasses by Captain Creagh-Osborne has led to great improvements in the radium compound, and also to a great development in its applications. There has now been organised at N.P.L., Teddington, a special department for the standardisation. In July, 1915, the three best samples available are shown by the certificate I have here to have had an actual luminosity or equivalent surface brightness of .0025 foot candle. The latest sample .0425.

This enables a card or dial to be read clearly at a distance of 3 to 4 feet. The light from the paint is an approach to monochromatic light being confined to the yellow green only.

A minus red screen is used in the tests at Teddington to reproduce radium effect. The definition of the radium lines is not quite sharp in the dark; there is an irradiation, which it has been suggested is partly due to the enlarged pupil.

The advantages of radium painted cards are:

1. The observer is not troubled by the glare of a lamp.
2. The compass is more portable.
3. The compass is always ready for use.

Hence its advantageous application to boats' compasses shown here.

4. All the divisions of the card are visible when held in the hand.

Discussion.

At the conclusion of Mr. Hughes's lecture, a series of Creagh-Osborne compasses with radium-painted cards were submitted to the audience. The following communication from Mr. Shackleton was then read by Mr. T. F. CONNOLLY:—

The question of the liquid to be used in a pocket prismatic compass is an important one. It is first necessary to fix on the conditions re-

quired. (1) A liquid giving sufficient damping appears the first criterion, but this is influenced by the size and depth of the box. The liquid should not be too volatile, and should have a low freezing point. It should not attack steel, though the magnet can be plated to avoid this.

If reference be made to the viscosity tables, say in the "Physical Tables of the Smithsonian Institution," it will be seen how different liquids vary in viscosity, and the vibration of a disc between a liquid is a measure of the viscosity of the liquid. Again, if the disc is so near the surface of the liquid that either it or the cap on the needle enter into the surface of the liquid, then the surface tension of the liquid enters into the problem, and everyone knows the damping effect of any vibrations in the surface of a liquid.

For this reason air bubbles should not be allowed to cling round the cap of the needle, and also the disc should be allowed to vibrate in a plane as far away from the surface as possible.

The tables of viscosity and capillarity suggest that paraffin oil might be a suitable liquid, but an analysis of two solutions taken from pocket liquid compasses showed one to contain a solution of glycerine and water, with 40 per cent glycerine; the other contained nothing but water and pure alcohol.

A few words on the historical side may not be out of place.

In his book on surveying instruments Stanley says the prismatic compass "was invented by Captain Kater about 1814," but I have a compass here which shows it to have been invented in 1811, since the dial is engraved, "Captain H. Kater, Invt. 1811." The instrument is made by Thos. Jones, whom, I take it, succeeded the celebrated W. & S. Jones, and is probably over 100 years old. It differs from the present-day compass in that the reading is made by means of a lens and a mirror, and not by the

usual magnifying prism; also the mirror holder, *i.e.*, prism box does not fold back, neither is there a check spring nor dark glasses; nevertheless, the needle is lifted off the pivot by a lever actuated by closing down the sight vane, as is done nowadays. Until about three weeks ago the original sighting mirror on this old instrument existed, but although it had come through the vicissitudes of a century, it was lost in transit across London, so recently Messrs. Troughton and Simms have kindly restored it nearly to its original form.

Very few years sufficed to bring the design of the compass to its present form, for F. W. Simms in his "Treatise on Mathematical Instruments," written in 1836, describes the instrument practically as it exists to-day. It is thus nearly a hundred years before any real improvement has been made, and even then the improvement is not novel.

MR. T. F. CONNOLLY, after reading the above letter, asked whether there was any objection to the use of glycerine for liquid compasses.

MR. HUGHES replied in the negative, adding that he had heard Sir William Ramsay suggest the use of glycerine, and it was sometimes used.

MR. CONNOLLY said he had imagined that a lack of parallelism would not have been a vital objection, and that if a clear image could be formed and the surface defects of the glass were not obvious so as to be evident in the image formed by the prism, that fact would not have been taken account of. But apparently at Woolwich they were exceedingly strict on the matter. He had with him a sample of glass taken from a rejected compass, and no doubt it was a fairly good prism, but the effect of that, of course, would be practically nothing on an image-forming prism above it, and the fact that the two surfaces were not strictly parallel he imagined would have quite a negligible effect on the reading of the compass. He was not clear, therefore, as to the real meaning of the rejec-

tion, and he would be very glad if any officer from Woolwich would enlighten him. He believed Woolwich was not so strict at the present moment, and that they were passing compasses very much more freely; but he thought the matter might be worth a moment's discussion.

MAJOR PURCELL (Royal Engineers' Stores, Woolwich) enquired the age of the glass in question, because, he said, no glasses had been rejected at Woolwich for eighteen months. The conclusion that it must be parallel glass had been arrived at in peace time. With regard to the question of the iridium point referred to by Mr. Hughes, many thousands of compasses had been passed at Woolwich (a large number of which had gone to Sandhurst and Cobham), and in the last three years only one had been returned from either place as becoming sluggish. He thought if the point was properly tempered it would not become sluggish. The question of the iridium point had been gone into very carefully, but the Government thought the extra expense was not warranted. He only spoke from the Mark VII. point of view, because he did not inspect many liquid compasses. He had not quite understood Mr. Hughes's point with regard to the moving of the prism if one wanted to take two or three readings.

Multiple Readings.

MR. HUGHES: Once the card is steady you can take several readings by moving the prism only.

MAJOR PURCELL: But you have to move the back sight as well as the prism?

MR. HUGHES: No, because a folding sight can be fixed on the prism.

MAJOR PURCELL: Otherwise you would not look through the prism.

MR. HUGHES: That is in the Mark VII., but not in the Creagh-Osborne, as the prism moves round the box.

MAJOR PURCELL said he understood that at

Teddington they were getting out an apparatus for testing so as to show exactly how much the instrument was out. They had found a great number of the compasses out of centre owing to the case being magnetic.

MR. ALEXANDER (Admiralty Compass Department) said he did not know that he could throw much light on the optical side of the question; he had come there rather as a humble learner wanting to get some light and leading from the members of that Society as to optical effect. There was one optical effect of radium which he had found in examining compasses, and that was that if the lens or prism was giving good magnification in daylight, it did not give good magnification at night when using radium. That was a point which he would be very glad to hear taken up by some of the members of the Society. It had puzzled him considerably at first. He had found himself suffering from a certain amount of eye-strain with the pressure of work and so on, and he began to wonder whether it was something personal; and without saying anything to the people whom he had drawn into his trap, he had just asked them to look at these radium cards in the darkness and tell him what effect they experienced, and had felt quite pleased from their answers to think it was not his poor old eye-sight which was going, but something in the radium which had a sort of pulsating effect. It was quite noticeable, the waxing and waning of the card, especially at first. That perhaps might wear off afterwards, but it was a thing he would like to hear discussed, and he had wondered whether it might be possible to evolve some sort of screen which would have the effect of making some prism or lens which would answer at night without being adjusted. Of course, by having an adjustable prism or lens one could always overcome that effect and get the lens or prism restored to the necessary focus for night work after using it for day work. The question of

the liquid for the compasses was very exhaustively gone into many years ago by an Admiralty Compass Committee, and the liquid which was now used was, after many exhaustive tests, adopted. That was a mixture of two parts of pure distilled water and one part spirits of wine, with a resulting specific gravity of between 96 and 97. Glycerine was used very largely by compass makers, but chiefly, he thought, for the sake of cheapness; and water, which would give the same freedom of action to the compass card and eliminate the drag which the viscosity of liquid had on a compass card, but it was a question which he never had had occasion to enquire into. He had never had experience personally with glycerine and water in compass bowls.

Defective Pivots.

The question of iridium pivots was one which had its difficulties, because there was iridium and iridium. Iridium was a very valuable substance, but unfortunately it did not always act in the same manner. Sometimes the pivot would splinter—a slice would splinter off the end of it, leaving a sharp, ragged edge, which, if the jewel was dropped on it two or three times, would sometimes have the effect of breaking the polished surface of the jewel, and which would often give no error in certain directions, but whenever the ragged edge of the pivot coincided with the damaged part of the jewel a very large frictional error was introduced at once. On other occasions the effect of concussion on the point of the iridium pivot was to "mushroom" it over, by which he meant that a ragged edge formed something like the edges of the mushroom. They had lately done away with having a sharp iridium pivot in all Admiralty compasses of that kind, and they always had the points of the iridium pivots slightly rounded, so that there was a very small frictional error, but if the card was not more than,

say, 60 degrees on the pivot, there was very little frictional error, and by rounding the point very slightly the tendency for it to get "mushroomed" over was largely eliminated. With regard to the prismatic compasses there was nothing new in any of the compasses which they had seen to-night except in the radium; in fact, his experience of compass work was that there was very little room for evolving anything new. He had in his museum at Deptford a liquid compass over a hundred years old, which, as far as he knew, had never been opened since it was made, and which he always held up as an awful example to all compass makers. So that as a matter of fact the only thing new in these compasses of to-day was the radium paint. Even the vertical card with the markings on was not at all new, for he had seen many specimens of that sort of thing—which showed that Solomon was wise before his time. (Laughter and applause.)

The Pupillary Aperture.

DR. STARLING thought the question of the use of radium paint in the dark would bear a little discussion, because a defect seemed to arise from the fact that it was visible in the dark, and under those conditions the pupil of the eye was, of course, largely dilated. Was not the focal length of the outer portion, the lensial portion, of the eye rather shorter than that of the axial portion? It had seemed to him at first, on looking at the card, that it was quite clear though not bright; in a short time the divisions became brilliant and their focusing was very imperfect; then he had tried with a diagram to cut out the other portions, and found that at a short focal distance the focusing decidedly deteriorated, and on blocking out the central portion there was considerable improvement. That seemed to show that what they were really using was the outer portion of the eye, which had a shorter focal length than that of the eye

as used under normal circumstances. That might account for the difference in the apparent size; they were really not using the normal eye at all, but a shorter focused and very much more imperfect one. He would like to ask a question with regard to the use of the two eyes, the scale being seen in one eye and the distant object in the other. Could it be guaranteed that the axes of the two eyes were parallel? Although one might get a general idea of the direction, an accurate bearing could not be obtained. Then, again, as to coming to rest with the compass, one point he had never seen attended to was this. What seemed to him desirable was that the compass should come to rest as quickly as possible. Now if it was oscillating freely it would take a long time to come to rest, and if heavily damped might never come to rest. He would like to know whether the attempt had ever been made to attain the critical damping. The relation was comparatively simple mathematically, but it seemed to him that would give the ideal compass, or, at any rate, improve it as much as it could possibly be improved. Perhaps some members who had worked in that line might inform them.

Fluctuating Focus.

THE PRESIDENT said he did not propose, merely because he happened to be President, to surrender his privilege, as an ordinary member, to join in any discussion, and as Dr. Starling had asked a question which seemed germane, he might be allowed to try and answer it. He, too, had noticed the difficulty of reading these luminous compasses at night. On ordinary nights, or in moonlight, the black figures on the white card were very clear and easily read. And they were most easily read, for some obscure reason, in the shop where they were bought. (Laughter.) But when one got them out at night they were all "fuzzy," and one wondered what on earth was the matter. One

could not focus the thing very well, though that could be done after a while. There were three physiological principles involved which were highly important. One was the impossibility of getting away from the knowledge that the disc was quite close to the eye, so that there was unconscious accommodation for a nearer point in spite of careful focusing of the convex surface of the prism. Beyond that, the floating of the card upwards and downwards caused differences of focus, and one endeavoured to overcome that fluctuation of focal length by an effort of accommodation. Now that introduced trouble number one: it caused the pupil to enlarge and contract, and that was the cause of what Dr. Starling had noticed, the fluctuation in the visual acuity. The second physiological point was a most interesting one. They had all no doubt noticed an electric light filament: when it was alight it seemed to be about one-sixteenth of an inch thick, and when the lamp was switched off it was so fine that it could scarcely be seen. But, of course, the filament was exactly the same size in both cases. Again, if one took an edge of anything and held it against a flame, that edge appeared to be notched by the flame, although they knew it was not so notched. These phenomena were caused by the curious principle of irradiation.

White on Black Type.

At one time there had been an endeavour to introduce black music into theatre orchestras because it was said that during certain scenes there was so much reflection from the musicians' white music-paper, and accordingly it was decided to print with white notes on black paper. But the musicians could not read this music at all; the notes were printed quite clearly, and were of exactly the same size as before, but they were illegible. This, again, was due to the same principle of irradiation, and he could give, if there were time, many other instances

of the same phenomenon. The nerve cells in the retina not only had to pass to the optic nerve and to the brain, but there were also what, in a more or less military company like that, he might call flank communications; they all kept touch with their neighbours, and when one was stimulated very strongly, the stimulation excited its neighbours, and thus they got a visual sensation spread over a larger area of the retina than was actually stimulated. And the third physiological point affecting this question of the card was that of adaptation. On going into a dark room to develop a photograph, and shutting the door, one could at first see nothing, but by and by one would be able to read a newspaper in the dim red illumination; and that adaptation of the eye to become sensitive in the dark was a third material factor at work in defining the compass card. (Applause.)

Mr. BOXALL (National Physical Laboratory, Teddington) said that with regard to the parallelism of glasses, if a limit of $5'$ was allowed in the refraction of the glasses it did not become a serious obstacle to the utility of the compass—it was not sufficient to be noticeable. It had to be rather a bad piece of glass that showed more than $5'$ of refraction. That had been for some time the limit laid down, formerly at Kew and then at Teddington. Of course, the extra expense of parallel glass was of importance. It was almost, one might say, unwarranted, as the line itself had some width, from about $10'$, or about 0.2 on the card, to somewhere about $20'$ when some distant object was taken in relation to it. So that $5'$ was rather a small consideration, and it was not a serious defect to use plane glass. As distinguished from parallel glass, plane glass, sometimes called plate glass, had two worked surfaces, which were sufficient to give clear definition. Parallel glass, of course, should have no prismatic effect at all, but when it was used in the cover it was so very near the

dial that parallel glass was rather a luxury there; but it would be an advantage to use parallel glass in the window provided that the line was fairly fine and small objects could be clearly seen. Probably the atmosphere in many instances was not clear enough to warrant it. That had been their experience in using the compass as it would be used in the field.

Lens Surface on Prism.

MR. BAKER wished to raise the point of the suitable power to put upon the curved surface of the prism. He had seen a good many of them, and it did not seem to him that there was any uniformity of practice in that respect. It was the fact of the wrong power being put on that curved surface which made it difficult to obtain a reading which one could trust to be free from parallax.

If one was taking a bearing of an object—take two objects on the horizon one degree apart, then the compass card was reflected up so as to come underneath.

As a general rule the compass card was magnified up, whereas the object itself was looked at natural size. The result was that one reading of the compass card would coincide with the object which was being looked at, and the next with that one degree away, but the card did not agree with the object one degree in actual angular distance from the first. That was due to the fact that too short focal length had been put on the curved surface of the prism, and the result was unnecessary magnification. Makers said, put a short focal length on for the purpose of magnifying up, but there was no object in doing that if the objects of which bearings were to be taken were looked at natural size. The proper thing was to have readings of the card subtending what was the equivalent of one degree as compared with the object which was being looked at direct. The suitable focal

length to make the curve on the prism was that of the radius of the card itself. If the compass used was of 1" radius, then the curve on the prism ought to be of 1" focal length, and then it would be perfectly possible to take a bearing whether the object looked at was against the lubber line or not; there would be no parallax when the head was moved slightly from the standard position.

MR. T. F. CONNOLLY said he was very much afraid he had forgotten the precise specification for prismatic compasses, but if he was not mistaken the magnification of the prism was given. He did not remember whether it varied in different compasses. He regarded Commander Baker's suggestion as a most excellent one, and would be very glad indeed to have any further light thrown on the point by any of the officers present who cared to do so.

MR. W. A. DIXEY said that if the glass was tested and a prism of, say, 5' was discovered in it, it could be obviated by the maker of the compass quite easily by turning the glass so that the prismatic error was parallel to the lubber line. Then the error, or the deflection, was only in a vertical direction and did not matter. It did begin to matter where there was a considerable error in a lateral direction, because then it interfered with the sights.

MR. T. F. CONNOLLY was afraid that that suggestion would not hold good.

MR. A. J. HUGHES suggested that it would be a valuable thing if some of the military gentlemen present would suggest the actual degree of accuracy they wanted these compasses to work to. The Woolwich test had always been half a degree, and the liquid compasses were made to that degree of accuracy, but it would be very useful if they could hear from some of the military officers present whether that was sufficiently close.

MR. BRYAN, referring to the peculiar effect of radium paint upon the eyes at night, said it

had been suggested by Dr. Ettles that irradiation had something to do with it. This irradiation, however, so far as he knew, was only strongly felt when the light was of considerable intensity, and the faint light from the markings on the card might not produce it. He would like to know whether anyone else present had had experience of that. Another point arose in connection with the question of focus. What was the wave length of the light? It was not, of course, monochromatic. If the wave length was very short, and it was a question of whether the outer or the inner part of the eye was being used, it might be useful at night to have a little glass which could be inserted so as to make the focus correct.

COLONEL PAGE said he had come there because he was interested in that particular compass. He had stumbled across it quite accidentally, and found that it had possibilities, from his own point of view as a field artilleryman, which seemed to him to solve many of the difficulties he experienced at present, when one had to take into the field a regular ironmonger's shop. His own experience was that he wanted absolute accuracy to half a degree, and if he could get that he was content. But if he were going to be anything like a degree out, then he thought he would prefer to put up with the ironmongery store which he at present had to take about with him. When a field artillery officer went out to take up a position he had to have a variety of men attending him, and had to cart about a big tripod with a director on the top, and also an angle of sight instrument. Unless one had a Mark III. the director was generally a separate instrument, and the compass almost invariably was, so there were generally three instruments to take about before it was possible to pick up the line of fire if one wanted magnetic compass work, which undoubtedly was an important thing, because by using such compasses for land work everything could be done

from the battery, and the officer need not be anywhere in sight of the enemy. He could be right away at the other end of a wood, or at the top of a tree, or anywhere. He supposed it was quite common knowledge that the lines of fire were not picked up by magnetic compass until quite recently, probably about two years ago. Having seen the Creagh-Osborne compass, he had gone up to have a talk with Mr. Hughes about it, and had made several suggestions to him. It had the great advantage, from his point of view, that it was steady, and with it one could pick up a reading very quickly. But it had certain disadvantages which he had pointed out to Mr. Hughes, because the ordinary layman liked to see a back-sight and a fore-sight; the ordinary artillery subaltern could not get out of his head that he ought to have two points to look to. He had therefore suggested to Mr. Hughes that it would be quite possible to put on the prism another length with a broad hinge, so that it should have no play on the other side, by which means a 3-inch line would be obtained which could be turned up as a fore-sight.

If that was put on quite a small telescopic stand that could be used absolutely as a director, the prism set at zero, the top plate set on the original line of fire, swung round, and so one would get the angle at once exactly the same as with an ordinary director. It would also be possible, without any trouble, without having to signal for an orderly, or shout across, to get the magnetic bearing automatically; it could always be got every time it was wanted. There were one or two minor details which he had explained that afternoon to Mr. Hughes, but it did seem to him that if those alterations were made the Creagh-Osborne compass could be made of very material value to the ordinary field battery; because the trouble at present was, as he had already said—and it was quite a common joke—that a man had to go into the

field with half an ironmongery store tied round his own waist and his orderly's waist, whereas he might simply have a small instrument strapped on to his belt not half the size of a revolver.

Demonstrating Luminous Dials.

LT.-COL. SYMONDS, referring to the apparent indistinctness of the radium paint, said a good deal depended on the conditions under which the radium-painted compass card was used. If the compass was taken down into a cellar for purposes of demonstration, it would probably be seen very indistinctly, but used in the field, when a man had been in the dark for an hour or more, it was found that sufficient light was afforded to enable a tram ticket to be read. He did not think it was a satisfactory test to suddenly turn out the lights and then attempt to use the compass.

THE PRESIDENT observed that it took twenty minutes in complete darkness before the eye reached the stage of maximum excitability.

LT.-COL. SYMONDS, continuing, said that some years ago an order was issued in the Navy that steel cap-stiffeners should no longer be allowed, and some experiments had been carried out at Shoeburyness with regard to the distance at which things affected the ordinary compass. It had been found that undoubtedly the steel stiffener in the cap affected it very much, as also did a very large knife, but the ordinary whistle did not affect it. Nothing, apparently, worn or carried below the waist affected it at all; and they had come to the conclusion as a result of those experiments that the magnetic compass should not be within a limit of 30 feet from an 18-pounder gun. It was very noticeable in the case of those ships which carried a 12-inch gun; the amount of steel in the 12-inch gun was alightly more than the amount of steel in the ship itself, and when they got to sea and started training the 12-inch gun the compass

trained with it. (Laughter.) In times of fog they had to be very careful to keep the 12-inch gun dead amidships.

THE PRESIDENT asked whether the compass was affected by the carrying of a revolver.

LT.-COL. SYMONDS replied in the negative. The experiment, he said, had been tried with a revolver, and apparently nothing that could be carried on the body below the level of the waist affected the compass at all. The experiment had been tried with the Mark III., the Mark IV., the Creagh-Osborne, and the Barker, and they all seemed about the same. But the cap-stiffener undoubtedly affected them all.

Magnetic Susceptibility.

MR. R. ARNISON, speaking purely from the manufacturer's point of view, and with some knowledge of compasses of all descriptions, did not think too much could be said upon the point of avoiding magnetism from foreign bodies. It depended entirely upon what position the compass was used in. He thought the reason it was not affected, when held right up to the eye, by a sword or revolver was probably that those objects were almost entirely below the compass. But if the compass was used in the hands and held sufficiently low down, then a watch in the waistcoat pocket would affect it. The smallest possible steel object, if brought sufficiently near, would have some effect on the compass, and that was a contingency which must always be guarded against. It was definitely laid down in the regulations, for instance, that if one was working on a railway line or in a railway truck, in order to neutralise the magnetic effect of foreign bodies the mass of metal must be equal on both sides of the instrument.

MR. ALEXANDER said he had found, in watching the effect of the radium-painted card from daylight, through the short twilight, into the darkness, the effect he had previously spoken of

was still perceptible, even after the gradual accustoming of the eye to the change from daylight to dark. He had spent several evenings in watching the performances of the radium from early twilight into the darkness, and the effect he had mentioned, the sort of pulsation, the waxing and waning, was still present. Nothing that had been said had quite cleared his mind on the subject.

THE PRESIDENT said the effect alluded to by Mr. Alexander was due to the phenomenon of retinal fatigue and recovery. If one took a card with chequers or dice on it, the one white and the other black, and gazed at that card for a sufficient length of time, one would find that, owing to retinal fatigue, the after image of the white degraded the white and the after image of the black was negatived and became white, and so the whole card became of a neutral grey, the design disappearing completely and both tints coming down to a uniform grey. Then, if the point of fixation was suddenly removed, the thing would suddenly flash up again. Looking at an object in the dark, a negative after image was superimposed on the point itself, and faded, but directly the fixation wandered the image came on a fresh retinal area and blazed up again. He thought that was really the explanation of the fluctuations referred to.

A Question of Focallisation.

MR. PURSER suggested that perhaps some of the difficulty which was apparently experienced in looking through the prism of the radium compass was due to the fact that there was no definite point or plane of light, but rather a radiance emanating from the radium, the depth of which varied according to the thickness of the layer and the quality of the composition, so that when looking through a short focus prism with its limited "depth of focus," one is unable to find a definite focal plane in the radiant

radium line. The President had referred to the use of the filament lamp. It was well known that almost the best method of testing the focus of lenses or binoculars was by the use of a filament lamp, the fine wire of which was seen after the irradiation was removed by the system of lenses. In the case of the radium compass, however, there was no one definite point or plane, but simply a luminous line formed of an indefinite number of planes to which the eye could not apparently accommodate itself, the result being that one did not get a sharp focus as when looking at the printed card whereon the lines are distinctly in one plane.

MR. HARRISON GLEW said he had been much interested in the question of radium paint. The question had arisen as to what was the wave length of the light given out by this luminous paint. He believed it to be practically monochromatic, though there might be a very, very faint violet line which was not seen at all in the ordinary way and need not be taken into account. The points mentioned by Dr. Etlles no doubt explained to a certain extent the effect which had been referred to, but there was in addition another defect, namely, the monochromatism of the light apart from daylight. What that was might be settled quite easily by interposing a screen by daylight. Certain coloured gelatines were very suitable for that. That which they were using in connection with testing paint at the National Physical Laboratory was a blue one, which very closely corresponded to the rays given out by the radium paint, and under those circumstances, of course, white light could be used as an accurate measurement through one of those screens, and the prism used accordingly. There was a very simple device used for doing that which he might mention. It was not so elaborate as the method used at Teddington. A standard lamp was enclosed in a box having an aperture in

which was a gelatine screen, which quite corresponded to the light given out by the radium, and in conjunction with that he used a sliding arrangement, consisting of a piece of white cardboard with a very sharp edge. The object to be tested giving out radium light was placed in the shadow of that, and it might be in a tube such as radium was bought in. It was quite easy to move either the object or the cardboard until it was possible to read within 4 per cent. Everything depended upon the screen. There were little pieces of gelatine used by draughtsmen for erasing faults, and if they were fortunate enough to get hold of one of those green screens it practically corresponded to the radium light ray. That was a very simple device which anybody could construct, and by its use it was quite easy to compare different samples of paint with one another.

Life of Luminous Paint.

A point had been raised about the duration of the paint, and he was sorry to say that from eleven years' experience he was able to assert quite definitely that it was not permanent. Formerly there had been no accurate method of measuring the durability of paint of this kind, but now it could quite easily be measured. In 1900 there appeared a paper by Professor Marsden, of New Zealand, on "The Measurement of the Decay of Zinc Sulphides," which formed the basis of these paints. By using a large quantity of radium the possibilities of response of the zinc sulphide could be exhausted in twelve hours, and in any case it was only a question of a shorter or longer time. However, that was not a very important matter, because the decay which took place did not affect the radium; the radium was always there, and could of course be recovered from the zinc sulphide base and used over again.

GENERAL STAVELEY proposed a vote of thanks to Mr. Hughes for his most interesting and lucid

paper, which was seconded by the President and carried by acclamation.

Reply.

MR. HUGHES, in acknowledging the vote of thanks, said he only hoped that some points had arisen out of the discussion which might prove useful. Mr. Alexander had truly said that there was nothing new in any of these compasses. All that he could say in reply to that was that they seemed to be new for military purposes. The liquid compass had seemed to him so essentially a nautical instrument that the introduction into the military sphere of points with which seamen were perfectly familiar might be useful. That was the only novelty connected with the Creagh-Osborne compass, except, of course, the use of radium paint.

MR. BAKER asked what was the composition of radium paint, and whether it was a secret. If not, what was the percentage of radium employed?

MR. HUGHES said zinc sulphide was the base, but he did not know the percentage of radium used.

MR. HARRISON GLEW said the Admiralty specification was 0.4 mm. radium to 1 gr. zinc sulphide.

THE USE OF GRATICULES IN BINOCULARS AND TELESCOPES.

By MR. S. D. CHALMERS, M.A.

Delivered 6th January, 1916.

THE graticule used in modern binoculars and other observing instruments has been developed gradually, and each step has marked an important advance in the construction of optical instruments.

The astronomer found it necessary to make use of a cross line at the focus of his telescope, particularly in his transit instrument. As a result the Ramsden eye piece was devised so that the cross line and the image might be equally sharp.

As transit observations became more important, additional cross lines were inserted at the focus, each cross line representing the image position when the object was on the line making a definite angle with the axis of the instrument. Thus the additional cross lines were used to fix definite angular departures from the main axis line of the instrument. The next stage was the use of stadia lines to measure distances.

If the lens B (fig. 1) form an image of the object O_1O_2 at I_1I_2 , the distance BO can be found from the relation

$$\frac{OB}{BI} = \frac{O_1O_2}{I_1I_2}$$

$$\text{i.e., } OB = O_1O_2 \times \frac{BI}{I_1I_2}$$

In ordinary practice BI will be *nearly* constant; we place two lines at I_1 and I_2 so that I_1I_2 is $\frac{1}{100}$ of BI , to find OB we place a scale at O , focus its image and note the length O_1O_2 on this scale which appears between the lines I_1I_2 . Then $OB = 100 \times O_1O_2$.

In practice the distance of I_1 from I_2 was adjusted by observing an object of known length placed at a distance equal to 100 times its length, *e.g.*, 1 yard at 100 yards.

It is evident, however, that BI may be appreciably greater for an object distance of 100 yards than it would be for a longer distance and the errors became appreciable when the stadia lines were adjusted on a short distance

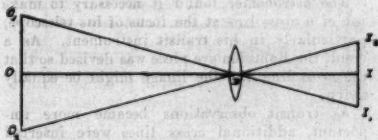


FIG. 1.

and used for longer distances. This difficulty is avoided by measuring all distances not from the lens but from its front principal focus.

In fig. 2, the ray O_1F is drawn from O_1 through F the front principal focus of the lens, to meet the principal plane of the lens at B_1 . It will, therefore, emerge parallel to the axis, and as this ray passes through the image point I_1 we have $II_1 = BB_1$.

$$\text{From the figure } \frac{OF}{FB} = \frac{OO_1}{BB_1} \text{ or } \frac{OO_1}{II_1}$$

$$\text{i.e., } OF = OO_1 \times \frac{FB}{II_1}$$

and by making $II_1 = \frac{1}{100}$ of the true equivalent focal length of the lens we have $OF = 100 \times OO_1$.

For convenience the diaphragm was often arranged to have a number of lines so that the factor 200, 100 or 50 could be used as the distance decreased.

This method may be applied to the problem of finding the length of a working base for a two position range finder by the aid of a graduated sub base.

The converse problem of finding the size of an object when its distance is known can be solved in a similar way. It is necessary to

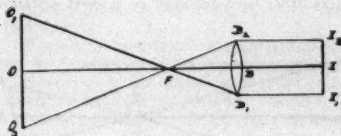


FIG. 2.

measure the size of the image of the whole object OO_1 by a graduated scale at the focus.

$$\text{We then have } OO_1 = II_1 \times \frac{FO}{FB}$$

and if the graduations of the scale are in $\frac{1}{100}$ of FB , we have

$$OO_1 = n \times \frac{FB}{100} \times \frac{FO}{FB} = n \times \frac{FO}{100}$$

where n is the number of the divisions of the eye piece scale which just cover the object.

Once more the distance FO is to be taken from the front principal focus to the object.

Sometimes, however, we know neither distance nor size, but are only concerned with the

apparent angular size of the object, i.e., of the angle O_1FO .

BF

If $BB_1 = \frac{\text{BF}}{57.3}$ then the angle BFB_1 will be

1° ; so that II_1 which = BB_1 is made equal to

$\frac{1}{57.3}$ of the focal length of the lens and I_1 is

marked as 1° . The other divisions are then taken proportionally.

To facilitate the division of the scale the focal length is often chosen so that the scale is readily obtained either in inch or mm. measure; e.g., prism binoculars $\times 6$ may be made of 114.6 mm. e.f.l. or 4.584 inches; 30 minutes = 1 mm. in the former case and $\frac{1}{30}$ inch in the latter.

BB_1

[Note. $\frac{\text{BB}_1}{\text{BF}} = \tan BFB_1$ and $\tan 2^\circ$ is not

exactly equal to $2 \tan 1^\circ$, but for the actual angles used no appreciable error is introduced though the error would become appreciable in very wide angle periscopes.]

Graticules may be used to observe errors of elevation; for example, the telescope is set so that the zero lies on the ground line of the target; a shell burst is observed to be so many minutes above the line.

Probably the most important use is to accurately locate a point whose approximate position is known.

Two observers at A and B lay the zero of their instruments in the directions which pass through the assumed position C and the object D (which may be a gun flash, a shell burst, or any other object) is noted to be so many degrees and minutes left or right of the axis in each case. These two observations enable the angles BAD and DBA (fig. 3) to be corrected from the known angles BAC and CBA, and the definite position of D is fixed.

If this procedure is to be adopted the grati-
cules should record in degrees and minutes.
There is, however, another method of reducing
the observations, and for this the graticule which
shows $\frac{1}{100}$ of the focal length is to be preferred.

In this case $AC = a \times 100$ yards and $BC = b \times 100$ yards will be known; the observer at A notes that D lies c divisions to the right of the zero and the observer at B notes that D lies d divisions to the right.

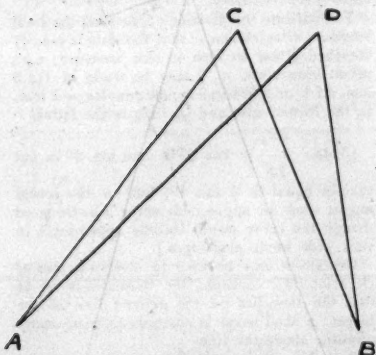


FIG. 3.

To find the position of D on the map. Draw (fig. 4) CF at right angles to AC and mark off $a \times c$ yards; draw CE at right angles to BC and mark off $b \times d$ yards; then through F draw a line parallel to AC and through E a line parallel to BC; these lines meet substantially in D, because AC and AD are nearly parallel and likewise BC and BD.

It is evident that this method of working may be used to find the position of a shell burst or

a fish, while it is also useful for filling in map details when leading points have been marked.

Fitting of Graticules.

The graticule must be placed at the focus of the objective but must be adjusted so that it is

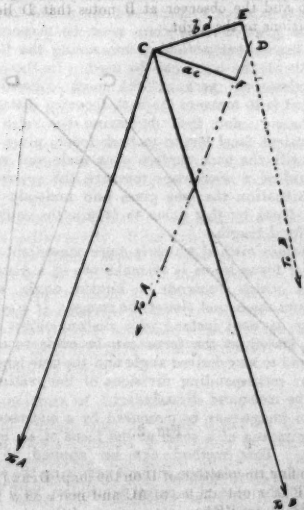


FIG. 4.

in the true image plane. At the same time the eye piece should be adjustable to allow any observer to focus it. The prism binocular with central focusing, and having one adjustable eye piece, is specially suitable. When the eye width of the binocular is variable the grati-

culé should be capable of rotation so that the divisions run horizontal and vertical.

When optical systems are designed to be used with a graticule it is desirable to adjust the focal length so that the graticule is readily divided to make either 1 mm. or $\frac{1}{16}$ of an inch a convenient division of the graticule.

But in many cases it is necessary to fit grati-
cules to existing systems and the actual length of the graticule divisions must be measured. Any convenient method of measuring the focal length of the o.g. may be used. In the case of telescopes, perhaps the most convenient method is to measure the back focusing distance of the o.g. and then determine the ratio of equivalent focal length to back focus; place the o.g. with the back surface on a scale and with the aid of a microscope measure the apparent magnification the lens gives and multiply the back focus by this value to obtain the equivalent focal length.

Another method which is more convenient for shorter focus lenses is to make use of a distant object which subtends a known angle and measure the actual size of the image. If a collimator be used instead of a distant object the scale placed at the focus can be made to correspond to any desired angle and the true length of the corresponding divisions of the graticule can be measured directly.

The image may be measured by a microscope or by means of a scale at the focus of an eyepiece. This method can be applied to a binocular or telescope without removing the o.g. and in the case of a prism binocular, with the prisms in position.

There are many other methods of finding the focal length of the lens and any of these may be used, provided care be taken to secure accurate results.

When neither a distant object nor a suitable collimator is available, the method used in adjusting the stadia lines of a level may be

employed to determine the graduations required, or to check existing graduations a stop is set up at a convenient place and a scale is placed square on and at a distance 57.3 times the convenient scale division from this stop.

This division should be chosen so that it can be subdivided as required.

Twenty-four centimeters, 12 cms., 9 cms., or 6 cms. may be used to represent 1° , and the corresponding distances would be 1.375 cms., .688 cms., .516 cms., or .344 cms. The lens is then brought up till its principal focus is at the stop. This principal focus is readily obtained when the eye piece is removed and it is only required roughly within $\frac{1}{2}$ or 2 cms., according as the shorter or longer distances are used.

The image is then measured and the focal length calculated or the graduations determined for 1° , etc.

The user of a binocular can check his graduations quite readily. If he uses the longer of the distances given above he need only place his binocular at 1.375 + 13 cms. from the scale and check whether the degree divisions correspond to intervals of 24 cms. on the scale.

[*Note.*—13 cms. are taken as an average value of the focal length of prism binocular o.g.'s in ordinary use.]

Manufacture of Graticules.

The rulings may be made direct on to the glass with a diamond, or they may be made on a wax covering and etched on to the glass. Photographic copies may be made of any graticule which has sufficiently marked lines and excellent reproductions can be made, provided that care be taken with the plates.

Contact prints of rulings as fine as 5,000 to one inch have been made by Lord Rayleigh and others on specially prepared plates, and I am exhibiting a specimen of these contact prints of rulings. I am also indebted to Messrs. Dollond and Messrs. E. R. Watts for the loan of

graticules and for certain special rulings which are on exhibition. Messrs. Sherwood are also, I understand, showing a few examples of rulings suitable for binoculars and machine gun sights. The accuracy and fineness of these rulings must be seen to be appreciated and I hope that members will examine those rulings both under low powers and under higher magnifications.

In conclusion I trust that these remarks will lead to a discussion in which those who have more acquaintance with the practical manufacture of graticules will take part.

ANNUAL MEETING.

10th February, 1916.

Introduction of New President.

THE President, Dr. Ettles, in introducing Mr. Frederic J. Cheshire as President of the Society for the ensuing year, said that Mr. Cheshire had played a very active part in the optical world for some time past. His activities had not been so much directly associated with the Optical Society as with one of the other societies interested in optical work in London, namely, the Royal Microscopical Society, and it was largely owing to his efforts that the development of the optical side of microscopical work was due. They owed to Mr. Cheshire a good deal of their present knowledge of matters connected with stereoscopic vision, polarisation, etc., and a number of instruments had been designed and developed with his assistance and co-operation. But perhaps more than anything else the optical industry was indebted to Mr. Cheshire from the fact that his work at the Patent Office had brought into the actual patent work in connection with the optical industry a rather different spirit to that which had originally prevailed. They owed to him the fact that when optical patents had been taken out of recent years, the patentee had been obliged to give information as to what was the actual thing aimed at, and care had been taken that the optical specifications should be as far as possible accurate descriptions of the apparatus in question, and this attempt to make all descriptions of apparatus scientific rather than purely commercial had been very valuable. He felt that Mr.

Cheshire was the right man in the right place, and he was sure they would all be very glad to have him as President of the Society, because they would all feel that the Society ought to be associated with work of that character. He had, therefore, much pleasure in introducing Mr. Cheshire as President of the Society.

MR. FREDERIC J. CHESHIRE then took the presidential chair. He said that when, a short time ago, he had been honoured by the invitation to accept the presidency of the Optical Society for the forthcoming year, he had been placed in a considerable difficulty. At first he had felt that he should not be justified in accepting any work whatever that was likely to interfere with his official efficiency; but upon considering the matter more fully he had come to the conclusion that he should increase his efficiency by accepting the invitation. (Hear, hear.) When the war broke out, the optical industry was in a most deplorable condition, due very largely to the fact that England had been suffering very severely from foreign competition for many years. There were very few first-class shops in the kingdom at the time, and this country was dependent to a very great extent upon Germany for its supplies of optical glass, so that when war broke out they were faced with an exceedingly critical situation, the problem, of course, being to supply the necessary optical munitions for the warlike operations at the front. Well, although at that time he had been pessimistic, knowing what he did, he was very glad to be able to stand before them on this occasion and say that the response of the optical industries of England to the demands of the last eighteen months had been simply magnificent. He had known of no finer instance of real British grit. Wherever one went now, in every district throughout the country, one found optical workshops all over the land, down in cellars, up in garrets, anywhere and everywhere, and most of these were doing really good work. He

would not say work of the very highest optical efficiency—that was not to be expected—but quite good enough for the needs of the present time.

A short time ago, in the North of England, he had gone into a workshop which before the war had been engaged in the production of certain projection apparatus. The owner of the shop had done his work so well that he had been able to compete successfully with Germany. When the war came his trade was shattered, and, as he had made enough to live upon, and he was no longer a young man, his first temptation was to retire. But he was anxious to "do his bit," so he came up to London, and, not disheartened by one, or two, or a dozen rebuffs, kept pegging away until responsible people came to the conclusion that the better plan would be to give him what he wanted and let him get on with his work. The final result was that at the present time he was doing munitions work, and a few weeks ago when he (Mr. Cheshire) went into his shop a most remarkable sight presented itself. Instead of the ordinary run of workmen, there were schoolboys, boys in knickerbockers, boys in Eton collars, and a number of girls—altogether the most extraordinary collection he had ever seen in a workshop. Upon making inquiries he found that the manufacturer in question had lost practically all his skilled labour at the beginning of the war, but being anxious, as he had said, to "do his bit," he had gone into his counting-house and turned out his clerks, gone into his woodworking shop and turned out his french polishers and joiners and so on, and gone into the schools and turned out the boys and girls at school, and set them all to work. He realised, of course, that such unskilled labour would be to a large extent useless except under very special conditions, but being himself an exceedingly clever mechanic and engineer, he at once set to work to invent special machinery which these unskilled people could successfully

operate, and, tackling the problem on these lines, he had met with very great success. At the present time that workshop was producing some of the best work in the kingdom, and, what was more, producing it to time. (Applause.)

That was simply one example of what was going on throughout the length and breadth of the land. Now the motto of the Optical Society for the ensuing year should be, not "Business as usual," but "Business well lost if the war well won." (Hear, hear.) He did not think any Society of that kind had a *raison d'être* at the present time unless it was doing war work, and that was, he understood, the position taken up by the Council of the Optical Society. In that connection there was a large amount of very good work which the Society could do. They were constantly faced with optical problems upon the solution of which depended production, and he had no doubt whatever that he should be able to come down to that Society and state those problems from time to time, and that amongst the members of that Society would be found many who would be both able and willing to take up those problems and successfully solve them.

DR. MULLINEUX WALMESLEY, speaking as a past president of the Society, moved that a hearty vote of thanks be accorded to the retiring president, the retiring members of Council, and the officers for the past year. Dr. Ettles had been president of the Society for two years, he had delivered two presidential addresses, and he was giving them a paper that night. He had worked very hard indeed for the Society during his tenure of the presidential chair, and he was sure that he would continue to take an interest in it and to work for it in the future. The same remark would apply to all the others to whom the vote of thanks was proposed to be given, and, although the motion was couched in the usual formal phraseology, he was sure it would be not less cordially adopted by those present

as a real expression of thanks for services faithfully rendered.

MR. T. VINYCOMB, in seconding, said that when he joined the Society rather more than a year ago he had been very much struck with the way in which the Society included in its purview all branches of optical interest, scientific, technical, and manufacturing. The discussions of the papers were real, and in cases where one was not clear as to how things were in regard to any optical matter, there was always an opportunity of getting such points cleared up. This satisfactory state of things was largely due to the efficiency and thoroughness of the president and the members of the Council, and he had great pleasure in seconding the vote of thanks. (Applause.)

THE PRESIDENT (Mr. Frederic J. Cheshire) then put the vote of thanks to the meeting, and it was carried by acclamation.

DR. ETTLES, in briefly acknowledging the vote, said he had greatly enjoyed the distinction of being President of the Optical Society. He had done his best to further its interests, and he was delighted to feel that those interests were now in such competent hands as those of Mr. Cheshire, who was above all men, in his opinion, most able to guide the work of the Society aright and to direct it into useful channels.

OPTICAL OR VISUAL SIGNALLING.

BY DR. WILLIAM ETTLES, PAST PRESIDENT.

An Address delivered 10th February, 1916.

Mr. President, ladies and gentlemen, our one preoccupation during the last eighteen months has been the waging of the greatest war in history, and our endeavour as a Society has been to assist in the solution of those optical problems which are important in warfare. The record of our past Session will amply bear out my claim, and, that being the case, no apology is necessary for the introduction of a subject which, on the one hand, has so many points bearing upon our special work, and, on the other, has such a large application in military science.

The idea that optical signalling was rendered obsolete by the introduction of wireless or line telegraphy, has been found to be quite erroneous. In the South African campaign we were told that many things had been learned subversive of the old canons. We were told that the magazine rifle was the one decisive arm, that the bayonet was obsolete, and so on. But a very short experience of the present war showed that these generalisations were too hasty. The prophets would have been rather nonplussed if they had been told that men would be going back to the hand grenade, the fluted morion, and even the old Roman catapult.

That spirit of hasty jettison affected the signal service, for after the South African war the old apparatus by which messages could be morsked—the lamp, the heliograph, and the flag-waving—were done away with, and the only means of communication between units was the hand semaphore during the day and despatch

riding at night. Fortunately many units maintained the old equipment, and kept up their morsing, with the result that very soon after hostilities broke out their ability to send messages proved of such high value that, eleven months after war had started, the Army Council issued an order that the old apparatus and

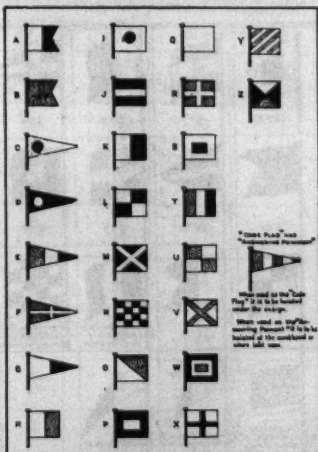


FIG. 1.—FLAGS AND PENNANTS TO BE USED IN THE INTERNATIONAL CODE.

- (1) "Code flag" to be hoisted under the ensign, and "answering pennant" to be hoisted at the masthead or where best seen.

methods should be reinstated. It reminds one of the Rousseau's dictum that "we advanced by retrogression."

Now optical signalling has many advantages. I have written a few down, and they are these: (1) Communication cannot be cut, as it can with all line telegraphy. (2) It is difficult to jam, and there it has a superiority over wireless at short ranges. Wireless is enormously valuable, and in its particular sphere is incomparable, but for short distances it has cer-

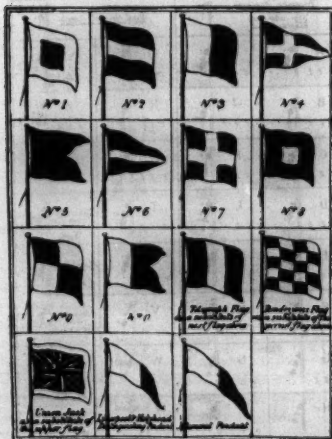


FIG. 2.—CAPT. MARRYAT'S FLAGS, 1820.

tain grave disadvantages, one of which is the ease with which it can be baffled or jammed, as when the "Goeben" escaped. (3) In optical signalling the direction of the message is restricted. In wireless, of course, a vibratory wave is sent out into space, which expands like a sphere in all directions, and can be picked up from anywhere, whereas in optical signalling, in most

cases, the path of communication is quite narrow, and directed to the receiving station only. *Apropos* of that, one cannot help thinking of Captain Müller's exploits on the "Emden," whence he kept calling, "Where is the 'Emden'?" when ships in his vicinity picked up

GENERAL ALPHABETICAL TABLE FOR MAKING THE INTERNATIONAL CODE SIGNALS BY MEANS OF VISUAL SIGNALS BY SHIPS			
Transmitting Station <i>Day, night and complete signal</i>		Receiving Station <i>Signal the other signal</i>	
A  1 1 2	I  2 1 2	Q  2 4 3	Y  3 2 3
B  1 2 1	J  2 1 3	R  2 3 4	Z  3 2 4
C  1 2 2	K  2 1 2	S  2 2 1	SPECIAL SIGNS
D  1 2 3	L  3 2 1	T  3 2 3	Code flag  1 2 3
E  1 1 4	M  2 1 3	U  2 3 3	Alphabetical  4 2 2
F  1 3 1	N  2 2 1	V  3 1 2	Alphabetical  1 2 3
G  1 4 2	O  2 3 3	W  3 2 3	Alphabetical  1 2 3
H  2 1 1	P  3 2 2	X  3 2 3	Alphabetical  1 2 3

FIG. 3.

and replied to these messages, he asked them to draw closer in order to give more details, and, when they came within range they were sunk. (4) The fourth point about optical signalling is its immunity from those breakdowns to which all delicate electrical instruments are necessarily liable. (5) With optical signalling

we have far greater mobility: the advancing fronts of the troops can be kept in touch with headquarters, and parallel columns can be kept in communication with each other. (6) Finally, there is the question of expense. Optical signalling is by a long way the cheapest method. The apparatus is simply "a stick and rag," so

THE BRITISH MOVABLE SEMAPHORE.

SEMAPHORE SIGNS.		GOVERNING SIGNS.								
 <i>Attention</i>		 <i>Precedence</i>	 <i>Alphabetical</i>	 <i>Number</i>	 <i>Signal</i>					
<i>Sign</i>	       	<i>A</i>	<i>B</i>	<i>C</i>	<i>D</i>	<i>E</i>	<i>F</i>	<i>G</i>	<i>H</i>	<i>I</i>
<i>Sign</i>	       	<i>1</i>	<i>2</i>	<i>3</i>	<i>4</i>	<i>5</i>	<i>6</i>	<i>7</i>	<i>8</i>	<i>9</i>
<i>Sign</i>	       	<i>J</i>	<i>K</i>	<i>L</i>	<i>M</i>	<i>N</i>	<i>O</i>	<i>P</i>	<i>Q</i>	<i>R</i>
<i>Sign</i>	       	<i>S</i>	<i>T</i>	<i>U</i>	<i>V</i>	<i>W</i>	<i>X</i>	<i>Y</i>	<i>Z</i>	

FIG. 4.

to speak; you can signal quite well with that, and in many conditions of warfare it can be adequately improvised.

Now there is one great drawback to optical signalling, and it is this one point that has given rise to so much ingenuity. That is, the constant conflict between visibility and concealment or secrecy. The more visible you make

your signal, the better it is from the point of view of being read; but the more visible it is to your friends, the more visible it is to your foes also. We shall see how these two absolutely antagonistic principles have been reconciled in a practical manner.

BRITISH METHOD OF SEMAPHORING BY HAND FLAG.						
						
2070						
Alphabetical Signification	A	B	C	D	E	F
Numerical Signification	1	2	3	4	5	6
2080						
Alphabetical Signification	G	H	I	J	K	L
Numerical Signification	7	8	9	10	11	12
2090						
Alphabetical Signification	M	N	O	P	Q	R
2100						
Alphabetical Signification	S	T	U	V	W	X
2110						
	Y	Z		A	B	C

FIG. 5.

My original intention was to describe the ordinary methods of signalling, because many members of the Society knew very little about it, and it occurred to me that if they were told how signalling was conducted they might take up the apparatus from the point of view of design, and, being expert manufacturers and good de-

signers, they might be able to carry that apparatus further from the point of view of efficiency. But when it became known that I was taking up this subject, I received so many applications from gentlemen who were interested, and who had far more practical acquaintance with the details of signalling than I could ever hope to possess, that the character of my audience became changed, and I realised that it would be quite foolish for me to give a lengthy description of ordinary Service apparatus to those who are daily using it. And, therefore, I will, if you please, touch upon this branch of the subject only very briefly, so that I may turn my attention to matters which will interest many of you much more—side issues, principles, and history.

Optical Signals in Peace Time.

Before I go into any detail about that, I would like to point out that optical signalling does not by any means obtain its only application in warfare. It has no mean uses in peace time. I have only to mention the railway signals. Railway companies spend anything from £100 to £1,000 a mile on their signals. So also in navigation, and buoyage, in the marking out of our coasts, and fairways, the organisation and usefulness is very great. However, I must pass those by, and deal more with military and naval signals.

If you study all the various methods of signalling, you will find that you can roughly divide them into three classes—first, those that by their mere presence indicate the condition of things. A good example of that is the wearing of a national ensign by a ship. That signal, by its mere presence, indicates a state of affairs. Then you have other signals which depend on their mechanical movement, such as the ordinary semaphore, the distance coast signal, to which I shall allude presently, and several other moving methods. Thirdly, we have signals

which depend on apparition and disappearance. The flash lamp and heliograph belong to that category. Now there an important subdivision comes in. You know that you can signal by means of a lamp in the ordinary way, long and short flashes, but there is another method

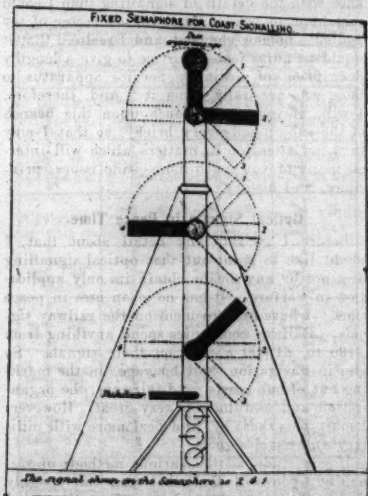


FIG. 6.

which has a particular use, and that is signalling by obscurations. In the telegraph service there are two kinds of sounders. There is the vibrator which gives the letter, or the portions of the letter, required by a series of long and short sounds, and there is also the sounder which gives the message by the intervals of silence.

We can adapt that to light. You can show a steady light, and make your signals by its obscurations, or you can make them by flashes. One method where this obscuring principle has been found an advantage is in the improvisation of heliographs. During the last stages of the

GENERAL ALPHABETICAL TABLE FOR MAKING THE INTERNATIONAL CODE SIGNALS BY MEANS OF DISTANT SIGNALS BY FLASH SIGNALS			
Flash after each signal		Flash the whole signal	
A	1	Q	Y
B	J	R	Z
C	K	S	SPECIAL
D	L	T	
E	M	U	
F	N	V	
G	O	W	
H	P	X	

FIG. 7.

Boer War the Boers valued the capture of a heliograph more than the capture of a field gun, because apparatus was getting very scarce, and they were improvising quite good heliographs out of ordinary mirrors. The expense and complexity of the heliograph depends largely on the extremely delicate movement which has to be

given to the mirror; when the button is pressed down the mirror is thrown into a certain angle, and that angle flashes the light on to the receiving station; in other words you are working a beam of light which normally is extending to 70, 80 or 100 miles away to the receiving station, and you have to get it on to the receiver's eye. Now a lever 100 miles long does not allow very much movement at the centre before it traverses a considerable amount of arc. By means of this system you can keep the light steadily on the station, making sure of their getting it, because you get a call immediately it goes off, and then, by having something like a ping-pong racket or a card, you can cut off the beam of light and so send messages by obacuration, without any contact with the mirror which would cause vibration, and therefore without requiring this delicate mechanical adjustment.

Flag Signalling.

Coming to details, I will first deal with the flag. The flag is a very ancient method of signalling. It may interest you to know that a red flag was mounted on the top of the Capitol in Rome when the Senate was sitting, just in the same way as, in peace time, we showed a light in the clock tower of the House of Commons. And the Guidon, which is a D-shaped flag, which you may have seen at the head of a cavalry regiment, has as its immediate antecedent the "Guide Homme." The Guidon is the "Guide Homme" of the old Norman French cavalry, and Cæsar's Legions had at their head a Vexillarius to bear the Vexillum. So that the cavalry standard goes back to long before the Christian era, and the little flag borne by the modern troop of cavalry has a very ancient history.

Signalling by Means of Flag Hoists or "Shapes."

I do not propose to give a detailed account of naval signalling. Since the principles involved

are just as well exemplified in the Mercantile Marine, I will derive my examples from the latter service. There is one flag for each letter of the alphabet, and one code flag also used as an answering pennant. There are two burgees or cornets, five pennants, and nineteen square flags. The designs are so chosen that the char-

SIGNALS.

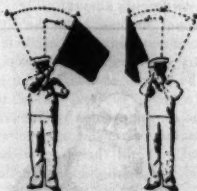


Fig. 1.

Fig. 2.

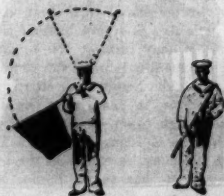


Fig. 3.

Fig. 4.

FIG. 8.

acter of the flag is not altered by hoisting it reversed. Many of these flags were designed by Capt. Marryat about a century ago, and those who are familiar with the International Code will be interested to see Marryat's own flags. They are numerical flags as originally used, but it will be seen at a glance that practically all are now used in the new code, and his third subti-

tute is our modern pilot-jack: the distinctive flag of the British Signal Manual.

Since it is the older method, I will first deal with the numeral flags and substitutes. This system was originally designed by the English astronomer Hooke, *i.e.*, numerals and substitutes, in 1684.

It had this advantage over the alphabetical flags, that fewer had to be used and learned. All

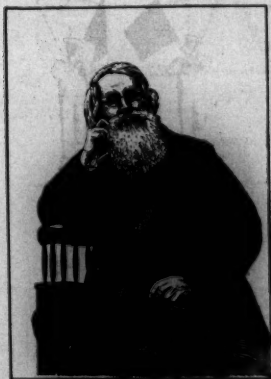


FIG. 9.—INVENTOR OF MORSE CODE.

that was needed was a flag for each digit, and one for zero—*i.e.*, ten flags instead of twenty-seven. In dealing with messages involving repetitions of numbers, *e.g.*, 4444, the locker would have to contain 40 flags, if no more than four is permissible in a single hoist. That was overcome by using a series of three flags called substitutes. In Marryat's flags they are modern T for the first, N for the second, and the pilot-

jack for the third. The old naval flags were a square red with white St. George's Cross for the first, a white and red pennant, red at the fly, for the second; and a horizontally banded tri-colour, red at the top, for the third. Thus only 13 flags were needed to send any desired message. Take, for instance, such a message as

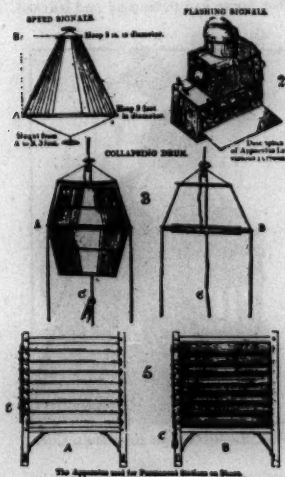


FIG. 10.

1111, 1212, 1221, 1123. The hoists are made thus:—

Flag 1	Flag 1	Flag 1	Flag 1
1111 S. 1	1212 Flag 2	1221 Flag 2	1123 S. 1
S. 2	S. 1	S. 2	Flag 2
S. 3	S. 2	S. 1	S. 3

The simplicity of the numerical system made it particularly applicable to flashes. When Admiral Colomb introduced his system, which was adopted by the Admiralty in 1867, he did not, as is generally imagined, use the Morse system. He employed a short flash for each digit up to five, and thereafter a combination of long and short (T,A,N,U,D). It was just as well it was so easily learned, for when Admiral Sir Sidney Dacres was ordered to report on Colomb's suggestion, he was allowed fifteen minutes to instruct a few signalmen, and had to conduct unexpected questions and answers the same night. Fortunately, it survived this ordeal successfully.

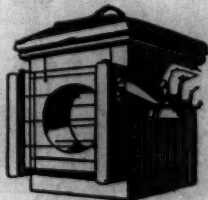


FIG. 11.

In connection with this numerical code, I may point out a method of making it secret. It is simply an application of the private word used by business people in marking goods for sale. Thus let such a word as "Codfishery" be the one selected. The signal 4632 becomes FSDO. It may also be used with the familiar commercial ABC Code. Thus "I have every reason to believe that I can get payment for hire to be made always one week in advance in cash. Will you let me know as soon as possible," can be sent in figures as 19466, 20462, 23033. It can be enciphered with this key word as CRFSS, OYFSO, ODYDD, and can thus be kept secret

no matter how many persons have access to the code book. It was Admiral Sir Francis Beaufort who introduced the key-word cipher.

The alphabetical system does not consist in simply spelling out a word. If you want to send out the message

OPTICAL

for example, you do not spell out "o," "p,"

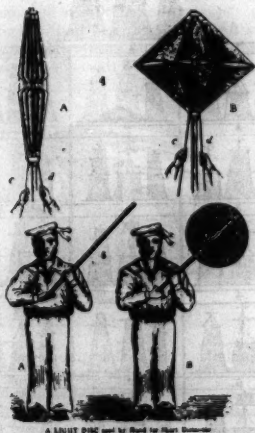


FIG. 12.

"t," "i," and so on, as many think. It is done purely by means of arbitrary combinations referring the receiver of the message to an idea in the code book.

FOI

means, "It is of no avail," and if you hoisted the signal "FOI" to a Dutch skipper, he would

look at his code book and see, "It is of no avail," in Dutch. And the same if he were French, or Spanish, or of any other nationality. So that you get this extraordinary position, that mariners of different nationalities, speaking to

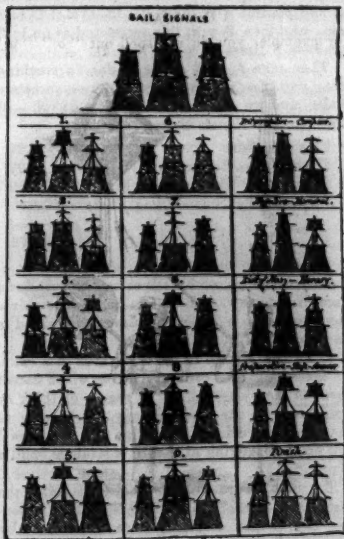


FIG. 13.

each other from a distance of miles by means of these signals, can tell each other anything they wish, and yet, if they were face to face on the same boat, they might not be able to say a word.

The code book is founded on the permutations of the 27 flags, using two flags for urgent and

danger signals. If we take one flag, A, and lay beside it the others in succession, AB, AC, A Code, AD, etc., we have 26, i.e., $N - 1$ combinations. If next we take B and make a series BA, BC, BD, etc., we have $N - 1$; and thus in all $N(N - 1)$ is the total of 2 flag hoists. Next take A and place it with the series BC to Z, CD to Z, etc., and then B with this series and so on to Z, we have N times $(N - 1)(N - 2)$.

Then take A to Z in succession in combination with the permutations of three successive flags, and we get $N(N - 1)(N - 2)(N - 3)$. The possible signals therefore are:—

27 single flags	...	...	...	27
27 × 26 (two flag hoists)	...	...	...	702
27 × 26 × 25 (three flag hoists)				17,550
27 × 26 × 25 × 24 (four flag hoists)				421,200

439,479

So that with these flags you see what an enormous number of combinations can be got, never exceeding four flags to each hoist, each combination conveying a different message, contained in the Code Book.

Flags may droop at their halyards in a calm, or there may be a contrary breeze which causes them to show edge on, or by reason of thick or hazy weather the observer may not be able to make out the colours. In that case "shapes" are used, consisting of a ball, a triangle, and a drum (fig. 3).

The ball occurs in every hoist, it being the distinctive sign of the distance signals. Colourless flag shapes were introduced originally by Captain Popham, afterwards Sir Home Popham, in 1784, but they were subsequently abandoned, and the present system of signals was instituted by Mr. Hall Gower, a naval architect.

The next slide shows the semaphore, with which most of you are familiar (fig. 4).

You see there are two arms movable on the

same axle at the head of a vertical post, and a little indicator sticks out at one side. When that is projecting it means that communication is open, and when it is shut it indicates that the message is finished. It also shows the side from which it is read. This machine was designed by Captain Pasley. It is often, although yet erroneously, called "Popham's semaphore." Semaphore is very rapid, and the individual letters are indicated by a single movement.

This represents the British method of signaling by hand flags.

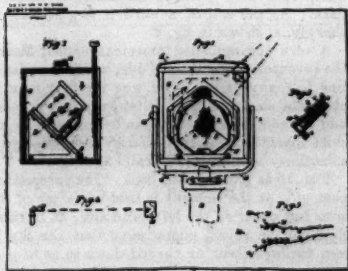


FIG. 14.—(GRUBB'S PATENT). See p. 80.

The next one shows the modern survival of Admiral Popham's semaphore, having arms separately pivoted on the same post.

The object of this, as distinct from the one previously shown, is that it can remain stationary for a long time, and is extremely visible at long distances. Hence it is called the Distance Coast Semaphore, and it is used instead of the International Code flags when the ship is too far out or the seeing is bad. There are only four positions for each post or arm and it is worked by means of these levers, which correspond to the positions of the arms.

This represents the various combinations of the arms, so that the system can be worked out either alphabetically or numerically.

Then fig. 8 indicates the Morse Code in operation, and to those of you who are not familiar with signalling it makes plain that while the semaphore, as you have just seen, depends for its efficacy upon the position of the two arms, the Morse Code depends upon long and short movements. The sailor in No. 1 is in a state of readiness; he moves his flag over the smaller angle either side to make a dot, and returns to his preparatory position in order to make a dash. At the end of a word it is gathered in sharply, as shown in fig. 4.

A very distinguished American named Morse, the inventor of the Morse Code, which is so familiar to many of you. He was the first man to apply the electro-magnet to telegraphic purposes, and the code which is thought by many to be his chief invention was regarded by him as quite a subsidiary matter. (Portrait, fig. 9.)

Fig. 10 is from an old book. The interesting part of it at the moment is that by means of the lever low down on the left-hand side, at B, these slats can be raised edgewise so that the sky is seen through them, or turned down so as to present a square obstruction, and the speed with which that shutter can be manipulated is very great, because each slat has only to turn through a short radius. This system is now adopted in all flashing lights. The oxy-hydrogen lamp used in the military service, and the arc lamp used in the Navy, are obscured or shown by this principle, which, as you see, is by no means new. Note also the collapsing drum, which is rather a curiosity. In the old days, by pulling a rope, this could be extended or shut up, and in that way apparitions or disappearances could be readily effected.

This represents the Cruiser Arc Lamp, supplied with its Venetian shutter. (Fig. 11.)

Fig. 12 shows you the collapsing cone. These

were very good methods of signalling indeed; they are very visible, and they are self-explanatory. The lower picture is rather curious; it represents the old signalling disc. That is the latest addition to our methods of military signalling, but it has an ancient history all the same, and the Navy was the first to employ it. It was fitted with a crutch handle which the operator turns so as to show edge on for disappearance and broadside on for apparition.

Fig. 13 is an old naval method of signalling when ships were a long distance away, long before the days of wireless, of course. The old

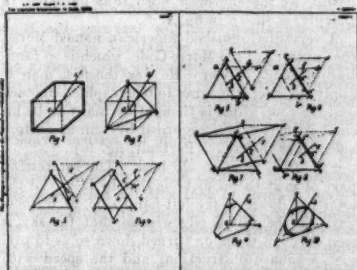


FIG. 15.—(ZEISS' PATENT). See p. 80.

three-deckers used two of their masts for signalling purposes; they used the two pairs of topgallant sails and two royals. When they wanted to signal, they used to send the topmen aloft, and they would stream out along the yards, gather up the sails all in readiness, and then they had to let them go in order. Sometimes they had to sheet them home if there was a wind, but if the weather was very fine they were just allowed to drop.

(A model was here shown of the signalling disc.)

This represents the modern military signalling disc, and its advantage over the old naval one is this—that whereas with the old one the disappearances and apparitions were equally visible to a spy or a hostile observer who happened to be anywhere in its vicinity, because the entire disc was rotated, with the new one the disc has in its surface a leaf or D-shaped panel, black on one side and white on the other. By turning this to one or other side, the whole disc seen becomes black or white, while the movement is imperceptible to any one at the side or in the rear. I strongly recommend that to volunteers especially, for the reason that just now there is a certain amount of unpopularity attaching to night signalling, and for learning the Morse code by means of a mechanical apparatus instead of a flag this signalling disc answers perfectly well; it can be practised in daylight, and there is no danger of being arrested as there is with lamp signalling.

I was going to refer to the optical problems of the heliograph, but I will not do that, because my friend, Mr. Shackleton, for one thing, is going to deal with the heliograph in a later paper, so the members of the Society will not lose anything by my omission; indeed, Mr. Shackleton has devoted so long a time to the study of the requirements of good heliographs that his remarks will be worth a great deal more than mine.

This looks like an astronomical telescope, but it is in reality "a signal gun" which has been made by one of our most esteemed members, Mr. Dixey. It is made entirely of aluminium, and at the lower left-hand end there is a little chamber hinged eccentrically, and this chamber contains a concave mirror with a lamp throwing a light upon it. The idea of having it movable about an axis parallel to the long shaft is in order that good centration may be obtained. Just near the left-hand end you will see a metal extrusion. It contains a series

of stops, and at the other end, the right-hand, or elevated, end, there is an iris diaphragm. The idea is to signal by diminishing its lateral visibility. I have tried to arrange a rifle with a little electric lamp where the bolt is, the signal being flashed by means of the trigger, and the operator keeping on to the receiving station by his sights, as in actual shooting. I still think

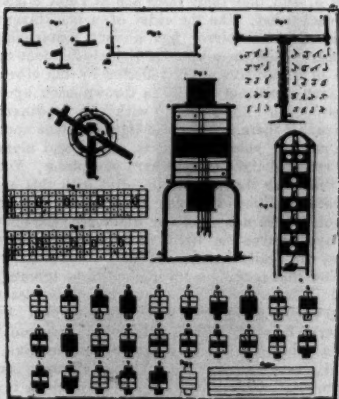


FIG. 16.

that a good way, but as you see, while I was thinking about it somebody else was doing it, and this is the slide which Mr. Dixey brought to me only this afternoon, and which I am very glad indeed to have the opportunity of showing to you. (Applause.) You aim at the receiving station with this signalling gun, and, of course, you do not need to allow for windage or elevation; you can take a point-blank aim and start

flashing away; no spy can pick up the reading, because the cross-section of the shaft of light is not greater than that of a rifle bullet.

This slide is from a specification by Sir Howard Grubb, of Dublin (p. 75). He took out a patent in 1903, and it is for what he calls a cube-angled mirror. This cube-angled mirror has three faces, and if you will look at fig. 1 you will see that the faces are numbered 1, 2 and 3, and that these faces are at right angles to each other, like the sides of a bricklayer's hod. Such a mirror has a very remarkable property. When a beam of light is incident on one of these faces, it is reflected by the others in such a manner that it is thrown back upon itself. You know that with an ordinary mirror, if there is a beam of light incident upon the mirror, you can turn it about, and make the reflected light travel where you please. You do that with the heliograph. But with this apparatus, if you turn the face of the mirror about, it does not make any difference; the light always returns on itself so long as all three faces are available.

Now that is too good a method to be ignored, and it is interesting to note that three years afterwards we find a specification filed by Messrs. Zeiss, the claim being that they could get a cube-angled mirror with a fluid medium. That was the difference: instead of using an air mirror, with three mirror faces, such as Sir Howard Grubb employed, the idea was exploited of making the cube angle of glass. It is exactly the same thing otherwise. Fig. 9 in Fig. 15, p. 77, shows the corners trimmed off. Fig. 10 shows where a circular face has been given to the prism by the rotation of the cylinder parallel to the principal axis.

The tetrahedral prism is a section of a corner of a cube at right angles to a line joining the diagonal angles of a cube. Suppose I sawed off the angle of that cube in the plane of that card-board, I could produce a triangular figure such

as you see there, and that would be a cube-angled or tetrahedral prism.

[The model shown was a cardboard box. A sheet of cardboard had an equilateral triangle cut out of it, and when a corner of the box was pushed through the aperture, the required figure was produced.]

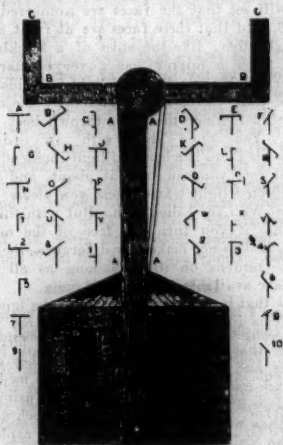


FIG. 17.

Here is one kindly lent me by Mr. Conrad Beck, and it is correct to $3''$ of arc, that is to say, the light is parallel, and the deviation is not more than the diameter of a halfpenny at the distance of a mile. If you look at it, you cannot keep your eye away from the centre; you can move it about as much as you like, but

your eye always irritatingly occupies the very middle of this reflector. Now that is a very fine instrument for signalling, because it is so absolutely secret. It is used in this way. I wish to communicate with someone a mile away, and I communicate with him by means of my own light. I hold up a lamp, and gaze alongside the lamp, and I pick him up; it does not matter where he is, as long as the face of the prism is directed in my own direction he need not align so long as the face is available, and I shall see a return flash from his prism. When he finds, by my raising or lowering the light, or some such means as that, that I have picked him up, he simply obscures the face and signals away to me a mile away with my own light. Now if an enterprising German spy put his head in the way he would not see anything because he would cut the light off, so that it is absolutely secret if it is well made. I hope you appreciate the point that Zeiss tried to "jam" this invention three years after Sir Howard Grubb. I may say that it did not "come off."

Invisible Light Signals.

Now I turn to a series of signals based upon the complex properties of light itself. You know that light varies, as to colour for one thing, and that visible light has beyond it, when spread out in the spectrum in the ordinary rainbow-like prism of colours which you are familiar with, other rays of light, at either end of the visible light, which are invisible. And the question naturally arises, Can we do anything with these invisible rays in the way of signalling and so on? Now the first thing that occurs to one with regard to these modifications of light itself is the colour. In the old days of railway signals, they used red for danger, green for caution, and white for safety. That was all very well, but so many white lights were about that engine drivers assumed that any white light they saw meant safety, and so ran

into something. Consequently they did away with the white light, because it did not work satisfactorily, and now, as you know, only red and green are used, and conversely, in a crowd of white lights, a coloured light is marked. We had some examples of that in the retreat from Mons, where Very rockets were being frequently sent up for the guidance of the Ger-

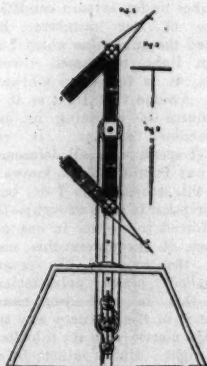


FIG. 18.

mans by spies in our lines. We baffled or jammed these signals by sending others of the same type from different areas. They did not know which one was the one intended as an indication. Another instance of the use of coloured lights occurred in the Zeppelin raids. I was in a high house in the West End of London at the time of the last raid, and I saw coloured lamps being shown from the roof of a house. These were bright red lights, and

were evidently being used to guide the Zeppelins. Of course, having only an isolated observation to go on, it was with great interest that I read that in Paris, on the occasion of the raid there the other day, they complained of the same thing, that the Zeppelins were guided by red lamps shown from the roofs.

Now I come to what is more striking still from the point of view of signals, and that is the case of an apparently steady white light showing flashes under certain conditions. Dr. Bryan, one of our members, has most kindly erected the apparatus which I now show you. When light is passed through certain crystals, it is forced to vibrate in one plane only. Assume that light as it emanates from the source is vibrating in an elliptical or circular manner. It encounters this crystal (I speak with all deference in the presence of our President, who knows so much more about this subject than I do, but at any rate my description is good enough)—(laughter)—and it is forced to vibrate in one particular plane. When it encounters this medium it vibrates in the axis of the crystal, and that is called plane polarisation. The beauty of this is that you cannot tell by looking at it in the ordinary way that there is anything the matter with it; it looks just like an ordinary light. But it is not; being polarised in the manner I have explained, when another crystal of the same sort is placed in the path of the light, the light passes through in the same plane of polarisation. As you see, when the crystal is rotated, the light is quite shut off. Let us turn this room into an eye, and imagine we are all inside it, the white screen representing the retina. That second crystal which the receiver holds in front of his eye, is slowly rotated, and we see that the light is extinguished. Then I suddenly interpose a plate of quartz, and you see a flash distinct enough to read a signal by. (Applause.)

Let that lamp be some miles away; it is still an ordinary steady white light. Then move up in front of it that little bit of quartz. There is not the slightest apparent difference. Of course we are here in this room, and we can see it, because we are almost close up against the light. But if you were far away do you think you would see that interposition? But let the man who is to receive the signal turn his analyser



FIG. 19.

round until the light is extinguished. When the signaller places this piece of quartz, which is perfectly transparent like glass, in front of it, the vibrations go through to the signaller's eye, and all the time the light is as steady as a bedroom candle. If you ask why are these ideas given away, the reply is that the Germans are quite as clever as we are, and they know all about polarised signalling. Does not all this throw importance upon some harmless coast-town lights, of which we have all heard so much?

I wonder if they have all been looked at by means of this apparatus? If not, they ought to be. What is the good of posting a special constable to see if there is any signalling?

There are several other methods of a like nature. I have here a piece of blue glass, which is remarkable in this way—that it transmits a very great deal of red light. If you hold this piece of blue glass in front of a point of light, and put on a pair of red spectacles, you will find that the point of light is a bright red. If you have another piece of blue glass which is not dichroic, you can shift the one blue from the

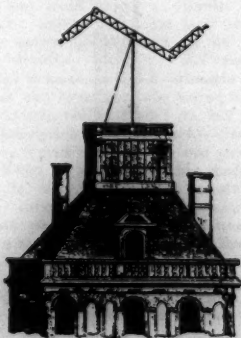


FIG. 20.

other, and flash away merrily to a man with red glasses, and nobody can see that signal unless he is close by. From any distance beyond speaking distance it certainly could not be "spotted."

Yet another method is this. Suppose you have a red glass placed at an angle to a green complementary glass, so that the light is trans-

mitted through the one, and the complementary reflected from its face.

The lamp will shine through that red screen and give you a red light, and the light will shine through the green screen and mix with it and give you a green light which will be reflected from the mirror-like surface of the red glass. In other words, the light will be white, but although it is white it consists only of red and green. Now the blue and yellow are also complementary, and if you have a blue plate and a yellow plate, and if you compare the light shining through the one pair with that shining through the other pair, you cannot tell the one white light from the other, because they are both white. But if you look at the first with a pair of red glasses you will see the red light quite well, while if you look at the second through red glasses you will see nothing but black. So all you have to do is to take these different coloured screens and pass them upwards and downwards, and, while all the time the light is white and steady, the man with the red glasses can read the signals nicely.

The other modifications of light are not so available for showing to an audience, and I will reserve them until after the lecture, so that those who are specially interested can look at them.*

The History of Signalling.

I will now turn my attention to the history of signalling, which goes a very long way back. One can quote Scripture on this subject, and, being a Scotaman, I naturally do so. We read in Isaiah (ch. xxx. 17) "To be left as a beacon upon the top of a mountain, and as an ensign on a hill." There you have clear historical evidence of beacon fires being used as signals. The siege of Troy (1184

* One or two of these methods are now under official consideration with reference to their practical use, and details are therefore omitted.

B.C.) is probably the first recorded piece of signalling in history, when the downfall of Troy was signalled by a series of beacon fires, and in an ancient Greek play there is put into the mouth of Chorus an expression of the incredulity of people at news travelling at such a speed. "Why," they say, "Troy is hundreds of miles

FRENCH METHOD OF SEMAPHORING BY HAND FLAGS						
						
22973						
Alphabetical Representation	A	B	C	D	E	F
Alphabetical Representation	1	2	3	4	5	6
22979						
Alphabetical Representation	G	H	I	J	K	L
Alphabetical Representation	7	8	9	0	1	2
22985						
Alphabetical Representation	M	N	O	P	Q	R
22991						
Alphabetical Representation	S	T	U	V	W	X
22997						
Alphabetical Representation	Y		Z	A	B	C

FIG. 21.

away, and you say that it occurred within a period when no messenger could possibly come!" Then Clytemnestra, the wife of Agamemnon, goes on to say how "Ida to Lemnos and Lemnos to Athos sent its beam of courier fire." It reminds one irresistibly of Macaulay's lines on the coming of the Armada:—

"From Eddystone to Berwick Bounds, from
 Lynn to Milford Bay,
 That time of slumber was as bright and busy
 as the day,
 For swift to east and swift to west the ghastly
 war-flame spread;
 High on St. Michael's Mount it shone—it shone
 on Beachy Head"—

incidentally showing how little we had altered
 in 3,000 years.

The Roman generals had a clever method of signalling. The legionaries were provided with water vessels, each furnished with a stop-cock, and floating on the surface of the water was a piece of cork, which in turn bore a board, and on this board a series of messages was inscribed. A number of these were placed in a row and standardised by timing. The water was suffered to escape for a given period, and the same message was written on each board at the new level. The vessels were distributed among the different regiments. The general had one beside him, and when he wanted to send a message he called his signaller and a light was shown. When the torch was waved and elevated, the recipients knew that a message was going to be sent, and stood by ready for work. As soon as that torch was elevated, the cocks were turned on, and as soon as it was extinguished they were turned off. The general watched his board until the water sank to the level of the message that he wanted to send, and the receivers read at the far station the message indicated on their own instrument. That, obviously, was a modification of the *clepsydra*, or water clock. The ancient Roman clock had a pair of little tanks, the floats in which actuated an hour hand. And when the Carthaginians invaded Sicily they actually used the *clepsydra*. They waited until the hand got round to the signal written on the dial, and then lighted a bonfire. Not only did the Romans communicate by means of water telegraphs in that way, but

they also had a cipher. Julius Cæsar tells us that he sent messages "*per quartam elementorum literam*"—that is to say, he chose every fourth letter. That was quite a good means but the Lacedæmonians were still more ingenious. When they wanted to send a message to a general, and it was to be kept secret, they used a cylinder of wood like a broomstick, and wound



FIG. 22.

upon it a piece of parchment spirally, after the manner of a puttee; they then wrote their message on the parchment in such a manner that the words were written across the intersections of the tape. One length of this wood cylinder was cut off and given to the general. When a message had to be sent, the ephors at home used to unwind this parchment tape, and hand it to a courier, who posted off to Africa or wherever the army was. The general re-wound the parchment tape on his half of the cylinder and as it was exactly of the same diameter as the other half, he was able to read the message. It seems

to me exceedingly likely, although there is no proof of it, that that device of the Lacedæmonians is the origin of our field marshal's baton.

The ancient Greeks had a very good method of signalling invented by Polybius. The Greek alphabet was arranged on five posts, thus:—

Α	Ζ	Λ	Π	Φ
Β	Η	Μ	Ρ	Χ
Γ	Θ	Ν	Σ	Ψ
Δ	Ι	Ξ	Τ	Ω
Ε	Κ	Ο	Υ	

A rough wall was built of stones, and on the other side of the wall were placed a number of men with torches, and these torches were held down below the level of the wall, so as not to be seen by the receiver. If they wanted to send "Α" they showed three torches above the wall to indicate that it was in the third column, and then away from that they would hoist one torch, showing that it was the first letter in the third column—"Α." In like manner, to indicate "Ω" five torches would be shown to indicate the fifth column, and then further along four torches, to indicate that it was the fourth letter in that column. Herodotus tells of an ingenious ruse, and as it relates to an oculist it may perhaps interest some of you. It seems that there was a conspiracy in progress, and a certain person engaged in this conspiracy wished to communicate with another, but did not quite know how to do so. At last a bright idea struck him. He got a slave who had sore eyes, and he said, "Now, my friend is a very great oculist, and I am sure he is going to cure you; but he has written to me that I have to prepare you." So he shaved the slave's head, and tattooed on his scalp the message he wished to send; then he kept him a prisoner until his hair had grown again, and said to him, "Take this message to the oculist: 'Shave my head and then employ your remedies.'" Off went the slave, and the other con-

He divided the alphabet into two groups, the first group comprising A to M, and the second N to Z. Under the first group he wrote "1," and under the second "2." Now, supposing he wanted to communicate "G," he would show a single torch, or "link," as he called it, seven times, while if he wanted to communicate "T," he would show two torches seven times.

The Marquis of Worcester, who was also an extraordinary sort of person, designed some signals about the year 1650. His method was to paper a window and put a bright lamp behind it; the different letters were cut out in cardboard or metal, and these silhouettes were held up against the window, thus he was able to conduct a correspondence, as he expressed it—"Though the night be dark as pitch is black."

I next turn to those signals dependent in movement which are so interesting. They came into being about the time of the French Revolution. For a time they created a great deal of wonder and admiration, until they all became "obsolete" by the introduction of the electric telegraph. Yet their history is very interesting, and I think that it will appeal to you as students of signalling and optics. The ancient Romans were able to semaphore, and there is distinct evidence of their having semaphores. Vegetius says*: "Some of them in castles, and towers, and towns, and cities, hang up beams, and sometimes these beams are shown erect and sometimes they are shown lowered, and they indicate what is going on." That is definite evidence that semaphoring was known to the Romans. Nothing further was heard of this method until Mr. John Edgeworth, about thirty years before the French Revolution, devised his system. He had a post driven into the ground,

* "Aliquantum in castellorum aut urbium turribus appendunt trabes: quibus aliquando erectis aliquando depositis, indicant quae gerunter."

and this post carried a triangular index hand which could be placed at any required angle. Nobody could make very much of it except the person who had this telescope. In the eyepiece of the telescope there was a cross-wire (this was about 1748) carried on a ring, numbered on the outside, and when the recipient saw a sig-

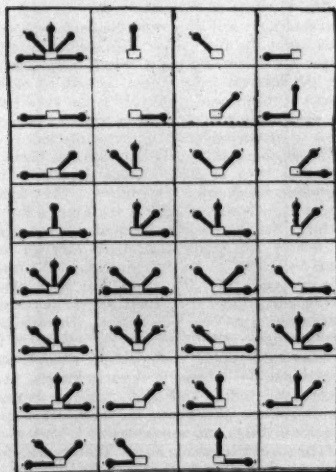


FIG. 24.

nal made, he turned the eyepiece of his telescope until the eyepiece wire was collinear with this triangle; he then found the number indicated and so got his numerical signal. A telescope of that description was in the possession of the Duke of York, and I believe was given to the Royal Laboratory at Woolwich. That was the

first mechanical telegraph ever invented, and an ingenious one it is, too. It was unquestionably invented thirty or forty years before the French Revolution. So that in strict historical accuracy, there is no doubt that to an Edgeworth belongs the honour of having invented modern movable signals. Still, in these times, we will not quarrel with our Allies, and if Edgeworth was not the inventor, then most certainly it was Claud Chappe.

Claud Chappe was an engineer, and he designed a method of distance signalling in 1792, and brought it before the National Assembly, who approved it in 1793, and established a series of stations, the first message sent being the news of the fall of Conde in 1793. I would like to show you, first, Chappe's apparatus.

It consists of a vertical post and a horizontal bar which can be moved see-saw fashion to any position, and which carries at each end a short arm. We shall see it more elaborated presently.

Fig. 16, No. 4, shows Edgeworth's apparatus. There is the arm, which can be placed at any inclination, and instead of pointing to numbers, in this case it is pointing to letters of the alphabet. That modification is for the convenience of the person sending the message, so that he knows he is in the exact position, and it is read off by a telescope with eye wires to check the reading of the letters.

Fig. 17 represents the Chappe telegraph; this actual illustration is from a sketch which was discovered in the pocket of a prisoner of war by an officer of the Black Watch who commanded the Scotch under the Duke of York, then Commander-in-Chief of the Army fighting in France. One author, I see, alludes to it as "the Prussian telegraph," but it is nothing of the sort. The letters are shown round about, and you see how, by the inclination of that horizontal arm, or "regulator," as they called it, you can obtain an enormous number of combinations by the movements of the short arm CB, which is

the indicator. Carlyle describes it in his immortal "History of the French Revolution"—

"Year 2, Messidor 3. What, for example, is this that Engineer Chappe is doing, in the Park of Vincennes? In the Park of Vincennes; and onwards, they say, in the Park of Lepelletier Saint-Fargeau, the assassinated Deputy; and still onwards to the heights of Ecouen and farther, he has scaffolding set up, has posts driven in; wooden arms with elbow joints are jerking and fugging in the air, in

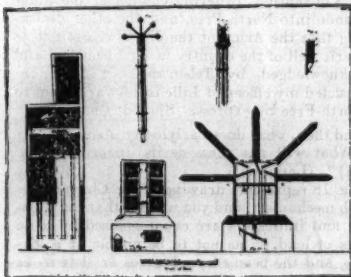


FIG. 25.

the most mysterious manner! Citoyens run up suspicious. Yes, O Citoyens, we are signalling; it is a device, this, worthy of the Republic; a thing for what we call 'Far-writing' without the aid of post-bags; in Greek it shall be named Telegraph. *Telegraphe sacre!* answers Citoyenism: For writing to Traitors, to Austria! and tears it down. Chappe had to escape and get a new Legislative Decree. Nevertheless he has accomplished it, the indefatigable Chappe; this far-writer, with its wooden arms and elbow joints, can intelligibly signal; and lines of them are set up to

the North Frontiers and else whithet. On an autumn evening of the Year Two, Far-writer having just written that Conde Town has surrendered to us, we send from the Tuileries Convention-Hall this response in the shape of Decree: 'The name Conde is changed to Nord Libre, North-Free. The Army of the North ceases not to merit well of the country.'—To the admiration of men! For, lo! in some half-hour, while the Convention yet debates, there arrives this new answer: 'I inform thee, *je t'annonce*, Citizen President, that the Decree of Convention, ordering change of the name Conde into North-Free, and the other declaring that the Army of the North ceases not to merit well of the country, are transmitted and acknowledged by Telegraph. I have instructed my officer of Lille to forward them to North-Free by express. Signed, Chappe."

And then, what does Carlyle say about it?

"What will not these devils incarnate contrive!" (Laughter.)

Fig. 18 represents drawing of the Chappe telegraph mechanism, and you will see that the regulator and indicators are counterpoised by little pieces of lead, so as not to be visible at a distance, and the beams have *louvres* or slots so as not to be so much affected by wind. The actuating mechanism in the watch room coincides precisely with the signal arms as seen by the distant station.

Fig. 19 represents a side elevation. By the way, the mechanism was designed by Mons. Breguet, the great horologist.

Later it was adapted for night signalling, and we see (fig. 20) the apparatus on the tower of the Louvre with Mons. Guyot's modification, five liquid hydrogen lanterns serve to delineate it for the transmission of messages.

Poor Chappe at last threw himself down a well, he was so disgusted with the many disappointments he encountered.

Fig. 21 is the French method of hand sema-

phoring; I reproduce it because there is a survival of Chappe's system in it. It looks at first sight nearly identical with our system of hand flags, but it is not so at all. Look at "I," for instance; in the man's two upper arms you have the regulator, and in his two vertical fore-arms you have the indicator. Such a signal as

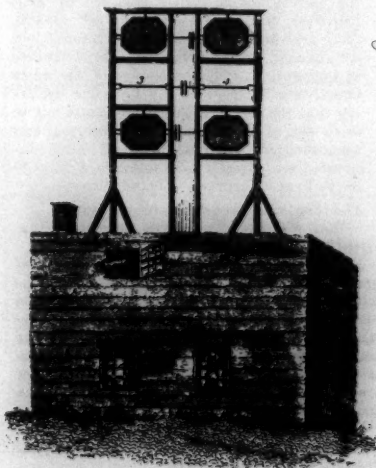


FIG. 26.

that could not possibly be transmitted by the Pasley semaphore, which we use, or in any method of signalling based upon it. Here you have a signal transmitted by exactly such an instrument as the Chappe telegraph, and if you look along you will find that in spite of the great

similarity there are many of the signals could not be sent by a single-pivot system. (Fig. 21.)

The next slide shows a Russian signal tower dated 1838. They were introduced by the Emperor Nicholas I., and he had lines of them all over his kingdom; they went right through to Siberia, and he could communicate from Petrograd to Moscow and from there to Tomsk. The personnel ran into thousands of officers and men,

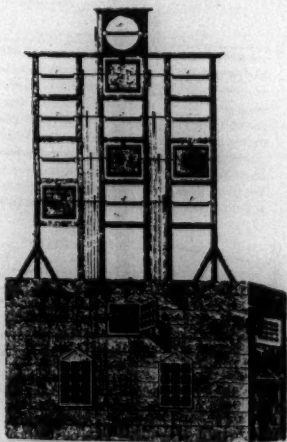


FIG. 27.

and the cost was enormous. These towers were very tall, so as to overlook the great pine trees of that country. (Fig. 22.)

Anent them, there is a curious story told by Ferdinand Hennicke, President of the Reichs-Postmuseum in Berlin.

"The Russians built high towers upon which the signal post with two transverse arms was erected, and with these conversation was carried on. The following is a true story which gives the palm to Russian telegraphists for conscientiousness. An official message was to be sent on the Siberian line from Petersburg to Moscow, to Tomsk, and the more remote

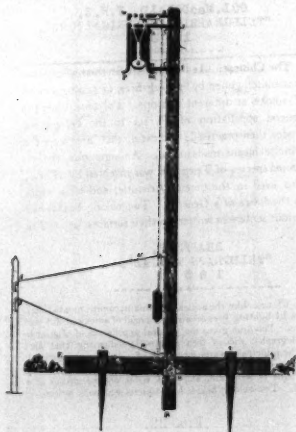


FIG. 28.

governors. It went without interruption as far as the Siberian frontier, but beyond that the telegraph arms remained rigid, and none of the curious signs which its European brother exhibited were reproduced. It came out afterwards that the Siberian attendant, as the result of a stiff smoke, was sleeping "the sleep of the just," and did not awake for

twelve hours. Then he saw at once with horror what had happened. Over yonder the European telegraph remained in dismal stillness making a signal which, with the upper arm exactly horizontal, suggested a gallows. The Siberian official shuddered. What would happen? Either execution or work in the mines. Instantly he puts his telegraph in posi-

COL. MACDONALD. F.R.S.,
"TELEGRAPHIC COMMUNICATIONS".
1808.

The Chinese, like the ancient Scythians, communicate intelligence by lighting fires, or raising a cloud of smoke at different stations. Polybius gives the general appellation of *Pyrria* to the telegraphic modes then practised; indicating that *fires* were the principal means made use of. An ingenious, though limited species of Telegraph was invented by *Aeneas*, who lived in the time of Aristotle, and who wrote on the duties of a General. Two oblong boards had various sentences written on their surfaces, as, "The

SHAFFNER.
"TELEGRAPH MANUAL".
1889.

The Chinese, like the ancient Scythians, communicated intelligence by lighting fires or raising a cloud of smoke at different stations. Polybius gives the general appellation of *Pyrria* to the telegraphic modes then practised; indicating that *fires* were the principal means made use of. An ingenious though limited species of telegraph was invented by *Aeneas*, who lived in the time of Aristotle, and who wrote on the duties of a general. Two oblong boards had various sentences written on

FIG. 29.

tion like the European yonder, and hanged himself on the upper arm. 'Remarkable signal,' thought the colleague on the second Siberian telegraph tower, 'but there is no help for it! the Czar must be obeyed.' Within two minutes this conscientious telegraphist dangled from the upper signal arm, and the sign was repeated from station to station as far as Tomsk. Through forty meridians of longi-

tude the whole lot of obedient Siberian telegraph attendants had to a man hanged themselves." (Laughter.)

Fig. 23 shows a signal observatory constructed from the designs of a sergeant in the Royal Engineers, and used by the Geodetic Survey of Canada in 1910. The tower is 87 feet high, and you will see how the high towers to which I have

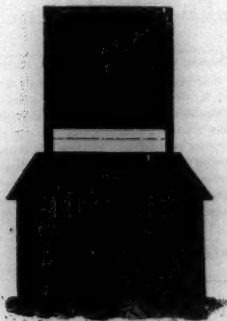


FIG. 30.

called your attention in the Russian system are repeated here, to overlook the Canadian pine. I think that is the highest signalling station that has been used of recent date.

The Rev. J. Gamble, Chaplain-General of the Forces, wrote a book on signalling in 1797, and fig. 24 is a page from it. You will see at a glance that many of our present-day signals are anticipated. He also designed the curious instrument actuated by a mechanism similar to that of "lazy tongs," termed the "radiated telegraph." (Fig. 25.)

It was taken by Sir Ralph Abercrombie to the

West Indies, but we have no account of its actual performance.

Fig. 26 is the old British "shutter" or "board" telegraph. On the top of a house six boards were so arranged that they could be turned edgewise or broadside on. This device was invented in 1796 by a Lord George Murray,

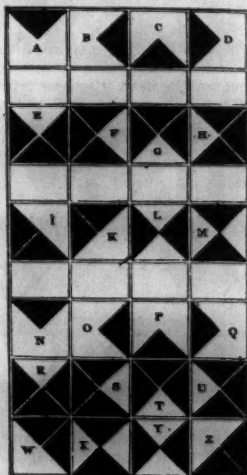


FIG. 31.—CONNOLLY'S TELEGRAPH ALPHABET.

subsequently Bishop of St. Davids. The invention was adopted by the Admiralty, and chains of these stations were established which placed the Admiralty at Whitehall in communication with Spithead, Sheerness, the Downs, Yarmouth, and other centres. The War Office had a network telegraph line. (Fig. 27.) A modification of this

system termed "the Syllabic Telegraph," was introduced by Colonel MacDonald (who, by the way, was the son of Flora MacDonald, of historic renown, the friend and sustainer of Prince Charles Stuart in 1745), but he made it much more elaborate, so that the number of signals was greatly increased; it never became officially adopted, as it was too complex.

He also introduced an ingenious flashing lamp. (Fig. 28.)

An extinguishing cover containing a lighted lamp, marked D, which was lowered by pushing up the counterpoise until it became visible, and

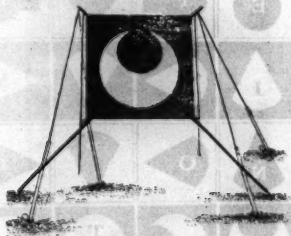


FIG. 32.

when they wanted to obscure the light it was drawn up into the bucket marked P.

When I was reading Schaffner's description of the early history of signalling, I came across Colonel MacDonald's little book, and I was so struck with the similarity of two passages that I have shown them simultaneously in the next slide, Fig. 29.

Why I think Schaffner deserves being exposed in this way is that he was always "down" on everything due to British efforts. Wherever he could take credit away from England he always did. (Laughter.) He never made any reference to MacDonald or acknowledged his obligation to

him in any way, yet he was not above "lifting" *verbatim* MacDonald's laborious researches in classical history. (Renewed laughter.)

Fig. 30 is Joseph Connolly's telegraph, introduced in 1808. It consists of a large square erected on the roof of a house. The figure is

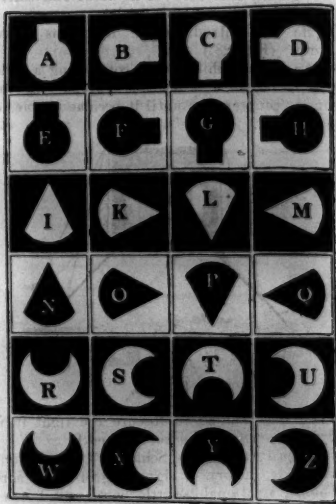


FIG. 33.

extremely visible, and the four black triangles were drawn away as occasion arose. (Fig. 31.)

Fig. 32 is his "field telegraph," and it is erected on two lances or sergeants' pikes, which are stuck crosswise, a useful design giving great rigidity of surface.

Fig. 33 gives the "shapes" he used, and I thought, after the long attention that you have given me, you might be interested in the next slide. (Fig. 34.)

Exhibiting the signal, "How are you?"—(laughter)—on the Connolly system.

This represents Admiral Popham's semaphore,

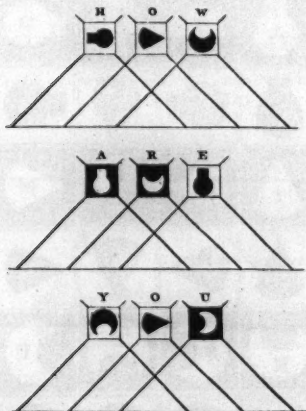


FIG. 34.

in which, as I have said, the arms were hinged separately on a post, and which superseded the old board telegraphs in 1816. (Fig. 35.)

It represents the station at Llysfaen, and I think you will agree that it did not do so badly when a message could be sent from Liverpool to Holyhead and back again, a distance of over 150 miles in 35 seconds.

Just to show how little we owe to "Kultur," I show you this slide (36). The Prussian tele-

graph system was introduced in 1842, and they simply "adopted" Popham's semaphore, needless to say, without acknowledgment. (Laughter.)

The Admiralty semaphore used to be well known in London. Leigh Hunt, in one of his essays, says:—

"Where a poor Archbishop sank down in horror at the sight of King Charles's execution, telegraphs now ply their dumb and far-seen discourse, like spirits in the guise of mechanism."

The whole system was swept away in 1851, when the electric telegraph came into being.

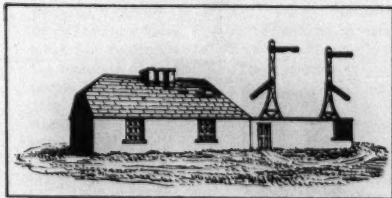


FIG. 35.

There is a story of an Admiralty official who said when the Crimean war was over, "They are going to have all the old telegraphs back again"; but I think that is a myth.

Now that really concludes my address. I am very grateful for the kind way in which you have listened to me. Of course I have not said anything like all I intended to say, but I have done as much as I could in the time, and I think some of you who are ardent signallers will have been pleased to see that signalling has an ancient history, and that in the old days its exponents had some ideas which are worthy of remembrance and utilisation.

I should like to add some words of acknowledgment. First and foremost to the Services. There is a general impression abroad that an enquirer or inventor who goes to the War Office or the Admiralty is received rather coldly, and that he soon becomes bound in a network of red tape. My own experience is that as soon as I approached the higher officials with a request that I should be accorded some facilities for investigating the history of signalling, not only was there no red tape to be found, but the utmost

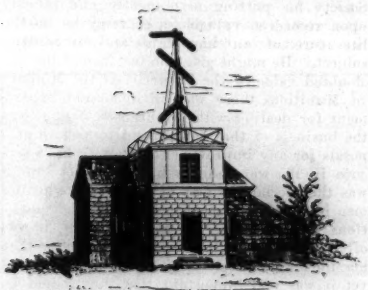


FIG. 36.

courtesy and consideration were extended to me. Nothing could have exceeded the kindness of the officers I encountered. Two especially I would like to mention, namely, the Director of Army Signals, and the Librarian of the Admiralty. Amongst our own members, Mr. Baker and Dr. Bryan of the Royal Naval College, Greenwich, have helped me very much, the former as touching the Patent Office records, and the latter in setting up the apparatus, so that the experiments might be shown. To Mr. Conrad Beck I am greatly indebted for the kind loan of the tetrahedral prism, Mr. Dixey for permission to

show the signal gun, now on its way to France, and to Mr. Sinclair and Mr. Snell for the trouble they have taken in making for me these fine lantern slides. (Loud applause.)

THE PRESIDENT said they had all listened with pleasure to Dr. Ettles' delightful lecture. What had struck him more than anything else in connection with it was the enormous amount of research it represented. He could not help asking himself, whilst the lecture was being delivered, what means were being adopted by the Society for putting permanently and quickly upon record so valuable a contribution to the literature of an important and interesting subject. He might give one instance of the undoubted value of the lecture. At the Ministry of Munitions there was an important department for dealing with inventions. Now it was the business of that department to receive proposals for any improvements likely to be of service in the war, and he had no doubt that it was the special business of one or more gentlemen in that department to deal with these questions of signalling. As they all knew, it was often very difficult to convince an inventor that his idea was merely rubbish—(laughter)—and yet it was very important that that should be done very quickly, providing, of course, the invention happened to fall within that category. He could imagine the position of an examiner in the Inventions Department if he happened to have at his side a copy of Dr. Ettles's lecture. He would merely cite "Dr. Ettles, page so-and-so," and the work would be done very quickly indeed. (Laughter.)

With regard to the cube-angled mirror exhibited by Dr. Ettles, he had himself had an amusing experience some years ago. He had had one of those mirrors placed upon his table, and, as they had heard, when one looked at it one saw an eye in the corner of the box. For some time his nerves had proved strong enough to stand it, but ultimately he had to give way,

for whenever he looked up there was this eye looking at him from the bottom of the box, until at last he had to send it away.

In connection with the prism, there was an exceedingly interesting problem, though he was afraid not one of much warlike value. Picking up a prism, you saw an eye in the corner of the box. Well, the first thing you did, of course, being an inquiring student, was to say, "Well, there is one eye, which is it?" (Laughter.) So you promptly closed the left. "Ah," you said, "it remains open, so it must be the right." If you were a very elementary student, you then considered you had solved the problem, and left it there; but if you were of a slightly more inquiring turn of mind, you said, "Yes, it must be the right eye, but I will confirm it; I will close the right eye." (Renewed laughter.) And then, to your amazement, you saw that the eye still remained open. The logical conclusion was that the eye you saw there was not the reflection of either of your own eyes, so you were at once faced with the very important question, "Whose eye is it, and which?" (Loud laughter.)

He concluded by moving a very hearty vote of thanks to Dr. Ettles, a proposition which was carried amid loud and prolonged applause.

A SIMPLE FOCOMETER FOR THE
DETERMINATION OF SHORT
FOCAL LENGTHS, BOTH
NEGATIVE AND POSITIVE.

By MR. T. F. CONNOLLY, M.Sc.

Read 9th March, 1916.

THE object set out to achieve is the measurement to a fair and sufficient degree of accuracy of focal lengths of positive lenses (chiefly single lenses), from $\frac{1}{2}$ to 3 inches focus, and also similar measurements for simple or compound negative lenses from $\frac{1}{4}$ to $2\frac{1}{2}$ inches in focal length. These measurements are not those required in careful design of optical systems but correspond rather to workshop requirements or students' laboratory work. In the case of the instrument for negative lenses, I find that an accuracy of one or two per cent. or better can be obtained with moderate care on simple lenses. This accuracy, I think it will be recognised, is adequate for most purposes.

In both cases the measurement of focal length depends on a measurement of the distance from the lens to the focal plane. It is, therefore, necessary to have some knowledge of the position of the "nodal points" or "equivalent points" of the lens under examination. For positive lenses the following cases must be recognised.

(1) **Plano-Convex Lens.**

In this case the lens should be so placed in the instrument that the convex side is turned towards the ground glass. This is theoretically very disadvantageous on account of "spherical aberration," but as the aperture of the beam is limited to one third of an inch in the instrument, the aberration can be practically

neglected. When focus is obtained in the position stated the focal length is measured from the screen to the apex of the curved surface and is at once read off in millimeters or inches on the scales provided.

(2) Double Convex Lens.

In the case of double convex lenses the measurement is no longer from the lens surface to the screen but from a point ("nodal point") within the lens. If the thickness of the lens does not exceed a few millimeters, this point may be taken as being one third of the thickness from the back surface. As the instrument records the

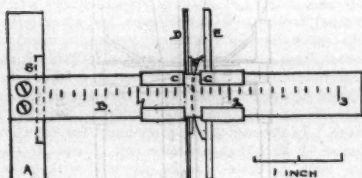


FIG. 1.

thickness of the lens under examination, this extra $\frac{1}{3}$ has only to be added to the reading given by the back index.

(3) Meniscus Lenses.

Here the nodal points are often outside the lens altogether and the "image-size" method, to be described later, is recommended.

From the above the limitations of the instrument will be clearly recognised but it is thought that its simplicity and convenience will be some compensation.

The positive instrument consists essentially of a 2-inch square wooden base to which are attached two bars. These bars guide and support the lens holder. This latter consists of two

thin plates of glass (E D) attached to metal slides. As will be seen from the drawing, the

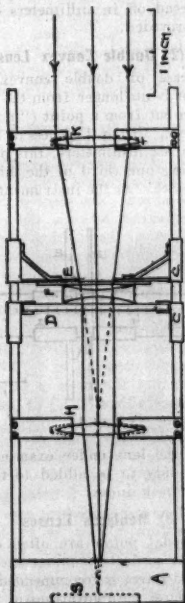


FIG. 2.

position of the two inner surfaces of the glass plates, and consequently the size of the lens enclosed, correspond to the readings of the two indices on the graduated bar.

The instrument is easily calibrated (or scaled) by using a lens of known focal length, and can similarly be checked at any time.

Thus scaled focal lengths, within the accuracy stated, are immediately available.

Without an optical bench or some rather elaborate or expensive apparatus it has not been possible hitherto to make measurements with any pretensions to accuracy of short negative focal lengths. The simple and handy optical system to be described allows this to be done with great ease.

A positive lens (K, fig. 2) gives a converging cone of rays. The next positive lens (H) is so adjusted that, if it receives parallel rays, or rays from a distant object, they are sharply focused on the screen. On placing the negative lens to be measured between the glass plates (D E) and adjusting the position along the bar, the converging beam is converted into a parallel beam as shown. This is easily recognised, as focus is once more obtained on the screen (S).

The distance from the point of convergence, or focus, of the first positive lens and the centre of the negative lens gives practically a very close approximation to the focal length. It is thus seen that the mean position between the two reading indices (C C) on the scales is the focal length. The above, of course, applies to symmetrical concave lenses the nodal points of which are always inside the thinnest central part and, therefore, practically central. The positive lens (H) now used is a corrected achromatic lens so that the converging cone of rays is without aberration. This is of importance when the negative focal lengths are relatively small.

In use the instrument is conveniently mounted on a base as shown in the photograph (fig 3) and the light from a workshop window, gas lamp, etc., situated at least 16 or 20 feet away, is directed vertically through the instrument by means of the small movable mirror below.

In cases where the positions of the "nodal points" are not evident both instruments lend themselves easily to a focal length measurement

which is independent of a knowledge of these positions.

In both cases the addition required is a small photographed scale of conveniently half mm. (approximately $\frac{1}{80}$ inch) which can be fixed on the ground glass screen. If a standard positive lens is available any focal length, within the limits of the instrument, can be found as the

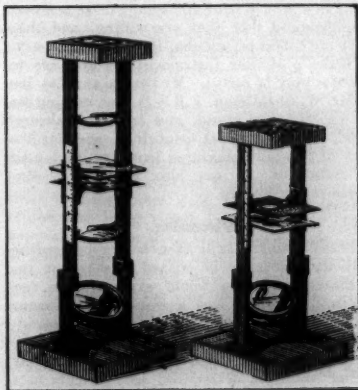


FIG. 3.

FIG. 4.

image sizes of any object are *directly proportional to the focal lengths*. With a standard negative lens a similar statement holds. In this case, however, the image sizes are *inversely proportional to the focal lengths*. The reason for this, as will be obvious from fig. 2, is that the negative lens under examination, in conjunction with the achromatic positive, forms a Galilean binocular and what we are really doing

is comparing the magnifying power of two different binoculars. The higher power is, of course, given by the system with the shorter focus negative lens.

The whole instrument has been designed so that its cost may be a minimum and thus it may be easily available to all.

I wish in conclusion to express my thanks to Messrs. Henry Hughes & Sons, opticians, Fenchurch Street, who have carried out my suggestions in a most skilful and sympathetic manner. I understand that they are willing, and they have my fullest permission, if that be necessary, to make a similar instrument for any one to whom it may be useful. My thanks are also due to Mr. W. Shackleton, F.R.A.S., for making me an experimental stand, and to Mr. Chalmers, M.A., for his general helpfulness and for the loan of carefully measured lenses which I used as standards. I am also grateful to Mr. Cheshire for his aid.

Discussion.

The PRESIDENT said that before proceeding with the discussion he wished to show two instruments which he would explain in a few words. The first was a workshop focometer made up from a simple rigid microscope stand. This was an instrument with which the true focal length of any lens not greater than four inches could be determined very quickly to a high order of accuracy. The essential part of the focometer was a collimator fitted to the sub-stage, after the suggestion of Mr. Blakesley. This collimator consists of a short tube with an achromatic lens of about one inch focal length at the middle. At the lower end of the tube in the anterior focal plane a glass diaphragm is inserted with two lines a millimeter apart, crossed by a fine line at right angles, thus making the ruled diaphragm to the shape of a letter H. In the upper end of the tube in the posterior focal plane of the lens a metal diaphragm is

fixed, made with a long narrow slit arranged parallel to the millimeter lines in the glass diaphragm below. The draw tube of the microscope is fitted with a low power objective at its lower end and a centimeter divided into one hundred parts in the eyepiece. In practice it is very desirable that the magnifying power of the draw tube microscope should be made equal to the focal length of the collimating lens in centimeters. When this is done each division of the eyepiece scale represents one millimeter of focal length. To determine the focal length of a lens it is only necessary to place it upon the microscope stage on the upper end of the collimator, and then rack back the microscope body until the two lines of the collimator scale—that is the two lines a millimeter apart—are focused on the eyepiece scale. The distance between the two projected line images in divisions of the eyepiece scale, is the focal length of the lens being tested, in millimeters. The apparatus may be employed either for negative or positive lenses. The image of the horizontal line on the collimator scale is projected with the aperture of the lens determined by the length of the slit already referred to for adjusting the position of a focal plane, therefore, the image of this line is observed.

The second instrument is an instrument for measuring back focal lengths in diopters and has been patented by Carl Zeiss on specification No. 12,100 of 1914. [This was reproduced in No. 1,304 of *THE OPTICIAN*.—Ed.] The instrument itself had been invented more particularly for dealing with spectacle lenses.

MR. CHALMERS suggested that some of the members might prefer to send in written communications rather than attempt to criticise Mr. Connolly's paper orally.

MR. EVERITT considered the application of an auxiliary positive lens to focus a parallel beam was particularly useful, and he was not aware that it had ever been done before.

SOME FURTHER NOTES ON FOCOMETRY

By MR. T. F. CONNOLLY, M.Sc.

Read 13th April, 1916.

I REGRET that I am not able to show this evening what I intended to show. Messrs. Hughes & Son, for good reasons in connection with war work, have not been able to complete for me the definitive instrument embodying several improvements. I had hoped not only to point out to you these improvements in the actual instrument, but also to present to you the results of my tests as to the practical accuracy obtainable. Mr. Chalmers had also promised to check the calibrations. I had hoped to have his results also.

I may, however, be allowed to suggest in a few words the alterations which I have made.

In the first place I have reduced the size of the aperture limiting the size of cone used to $\frac{1}{2}$ inch. I had found this to be sufficient to give fair certainty of focus, and, of course, the aberrations are diminished.

In the case of the negative instrument the plain glass supporting the lens under test has been abolished, and the slight error introduced thus eliminated. A much thinner glass has also been used in the "plus" instrument.

The mechanical rigidity has been improved by strutting all the sliding lens holders.

MR. CHALMERS suggested that Mr. Connolly might obviate the difficulty of that focusing error by using an annular aperture in front of the lens. The focus would be more definite in that case, and there would be less liability to error on account of errors in the plus lens.

MR. CONNOLLY: Use the annulus of the first lens?

MR. CHALMERS: Yes. I think you will get rid of most of the errors in that way.

MR. CONNOLLY: Yes, I quite agree that the error would be decreased, but in the instrument under question as any part of the cone of light may be required to be used, the modification is quite impossible.

ON THE MANUFACTURE AND TESTING OF PRISMATIC AND OTHER COMPASSES.

MR. F. E. SMITH, A.R.C.Sc.

Read 9th March, 1916.

THE primary object of this paper is to assist makers of prismatic compasses in the manufacture of these instruments and in testing their reliability.

The knowledge that a compass has to pass certain tests is of no assistance to the maker unless details of the tests are supplied to him. If the reasons for the tests are known it is, in general, not difficult to remedy any defects. Assuming such knowledge, it is customary for the instrument maker to subject his instruments to similar tests in his own workshops, and he eventually produces a very reliable instrument with little or no increase in the cost, thus giving greater satisfaction both to himself and to his customer. In the past, such tests have not been made by the maker, or, if so, to a trifling extent. One great stumbling block has been the erroneous idea that a non-magnetic hut is essential for the proper testing of prismatic compasses. I hope to show that this is not so.

While my remarks are applicable to compasses in general, I particularly deal with the military service compass. Verner's Pattern, Mark VII. That part of the paper dealing with "damped oscillations" will, it is hoped, prove of use to makers of liquid compasses, especially ship compasses.

With regard to the tests, I deal exclusively with those made at the National Physical Laboratory.

As the object of a prismatic compass is to determine direction, any defect in material or error in workmanship which affects the deter-

mination of direction should be tested for. It is also an essential condition that these tests should not be merely of a qualitative nature, but should indicate the magnitude of the error accruing from faulty material or bad workmanship.

The possible errors may be briefly summarised thus :—

(1) Error due to want of parallelism of axis of magnet with the 180° — 360° direction as indicated on the dial.

(2) Error due to eccentricity of dial and point of suspension.

(3) Error due to centre of suspension not being in plane containing slit and line.

(4) Error due to want of uniformity of the engraved divisions on the dial.

(5) Error due to sighting line being out of the vertical when the compass is supported on a horizontal surface.

(6) Error due to prismatic effect of the glasses (window and cover).

(7) Error due to magnetic material in the case, screws, etc.

(8) Error due to faulty setting of prism in the prism box.

(9) Error due to excessive friction between pivot and jewel.

Possible Errors Nos. (1), (2) and (3).

First with regard to the errors (1), (2) and (3). Suppose that the magnet is placed with its axis parallel to the 180° — 360° diameter, but that its centre is not over the centre of the dial, as in fig. 1. Then if O is the centre of the dial and also the centre of support it is apparent that the bearings given for the directions N., S., E., and W. are all accurate, and a little consideration shows that all other bearings are also accurate. Hence it is not *essential* to have the axis of the magnet directly under the 180° — 360° line.

If, however, the axis of the magnet is not

parallel to the $180^{\circ}-360^{\circ}$ diameter the error of the bearings (in degrees) will be constant and

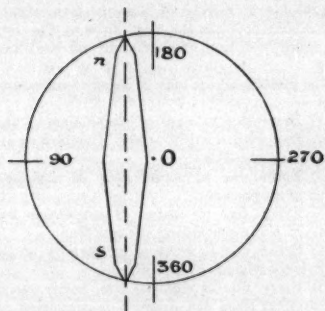


Fig. 1.

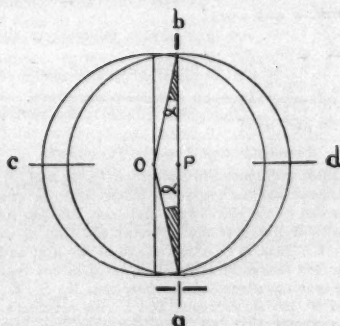


Fig. 2.

equal to the angle between the magnet's axis and the diameter in question.

Next suppose that the centre of the dial as represented by the engraved divisions does not lie on the axis of support, so that in plan (fig. 2) O represents the dial's centre and P the centre of support. Also let the latter be in the vertical plane containing line and slit. Then readings in the directions a and b will be in error by $+\alpha$ and $-\alpha$ degrees respectively, and readings in the directions c and d will be correct. In directions between a b and c d the possible errors are less than α .

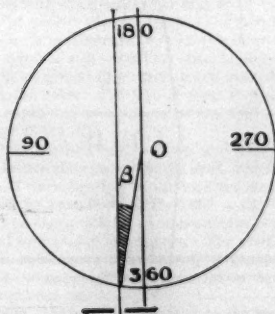


Fig. 3.

When the vertical plane containing line and slit is parallel to, but does not contain, the $180^\circ - 360^\circ$ diameter of the dial (fig. 3), but the centre of the dial is on the axis of support, the error is constant and always of the same sign. In the example shown in fig. 3 the readings are everywhere too great by an amount β .

In general the bearing errors (1), (2) and (3) are present to a very small extent in every compass.

On inspection of a large number of prismatic

compasses I find that the axis of the magnet is frequently inclined to the 180° – 360° line, errors as large as 3° being due to this cause alone. With regard to the eccentricity of the dial and point of support I find this to be very small in compasses of some makers and very large and variable in others. I have measured



FIG. 4.

this eccentricity by mounting a dial on a pivot and noting the displacement of one of the engraved circles on rotation of the dial. If the centre of the circles is displaced eccentrically from the point of support by 0.4 mm. the error introduced in the reading on a dial 45 mm. in diameter varies from $+1^{\circ}$ to -1° . The error

due to this cause in most of the compasses examined was about half a degree.

It appears that errors (1) and (2) would be considerably reduced if the following process of construction were adopted:—

(a) The magnet pieces to be stamped and of regular shape, the three circular holes $C D C'$ (fig. 4) being on the axis of the magnet. These holes to be tapped. There is, therefore, perfect symmetry about the line $a b$. When these steel pieces are hardened and properly magnetised (see later section of paper) the magnetic axis coincides with the geometrical axis $a b$.

(b) The mother of pearl disc to have three holes drilled in it corresponding with the three holes in the magnet, and when the disc is engraved it should be placed over three studs protruding through the holes. The line of these three studs on the engraving machine to be the $180^\circ - 360^\circ$ diameter of the dial.

(c) The jewelled centre piece and two small screws secure the magnet to the dial, and if perfection of workmanship be assumed the magnetic axis will be parallel with the $180^\circ - 360^\circ$ diameter and there will be no eccentricity between the dial and centre of support. The mother of pearl is sufficiently transparent to enable one to see if the points of the magnet lie on the diameter referred to.

There is a chance of eccentricity in the jewelled centre piece, and in assembling, care should be taken to avoid this. The dimensions of the piece are so small that with ordinary care in fixing the jewel the error due to it is negligible.

The possible errors (1), (2) and (3) are not tested for separately, but the combined error is obtained by measuring the practical effect on the bearing readings when the line is sighted in magnetic N., S., E., and W. directions.

Errors of bearings are measured at the National Physical Laboratory in a special hut built of non-magnetic material and in which the direction of the earth's magnetic field can at any

moment be determined within less than a tenth of a degree. In one portion of this hut a declinometer with a mirror is set up, and the position of a line of light on a transparent scale gives the magnetic declination. On special supports in the hut prismatic compasses can be tested. Readings of the compasses are taken when the plane containing the slit and line lies in the meridian, and other readings are made when this plane is at right angles to the meridian.

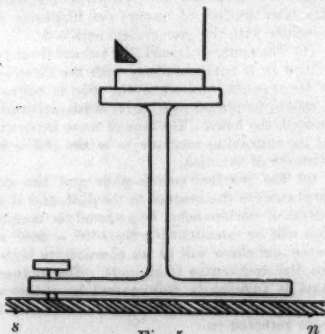


Fig. 5.

In all, four readings corresponding to the directions magnetic N., S., E., and W. are taken, with a probable error due to the arrangements of not greater than a tenth of a degree. With liquid compasses readings are made every 45° .

I do not propose, however, to describe these arrangements in detail. What I wish to do is to indicate how the manufacturer can determine the bearing errors for himself.

In the past the great difficulty has been the fluctuation of the magnetic field in any ordinary workshop or test-room. In one case a manufac-

turer assured me that the daily change in the direction of the magnetic field in his test-room was normally 3° ; this variation was largely due to some neighbouring electrical machinery.

Fortunately the difficulty is readily overcome, and provided that the maker has one good standard compass he can easily and cheaply find all bearing errors for himself. I have devised several ways of doing this but here I describe what I think is the best way. Suppose a good prismatic compass to be mounted on a small table as shown in fig. 5 and let it fit into a ring of wood or brass—or better let there be a centering device—so that other compasses may be substituted for it with a certainty of occupying the same position. Turn the compass until the reading is approximately 360° and note the time of swing as well as the reading of the dial. Let the time of swing be T_1 seconds. In the figure a simple brass pedestal screwed to a rigid support is shown supporting the compass. Under the base of this pedestal it is possible to introduce a long bar magnet the poles of which are *n s*. The height of the pedestal above the table need not be more than 15 cm. After the period T_1 and the reading of the dial have been taken the long bar magnet is introduced into position (*n* pole pointing north), so that the centre of the magnet is practically under the centre of the compass and its axis approximately in the magnetic meridian. To ensure the latter condition being satisfied the direction of the magnet is changed until the reading of the compass is the same as before. Again, take the period of the compass. Let it be T_2 seconds. Then the new magnetic field intensity is greater than the old magnetic field intensity in the proportion T_1^2 to T_2^2 . Thus if the first time of swing was 6.0 seconds and the second 2.0 seconds, the second magnetic field is nine times as intense as the original field. With the arrangement sketched (the compass being 15 cm. above the level of the table) I have obtained a magnetic

field 10 times as intense as the earth's horizontal field by using a bar magnet 25 cm. $\times$ 2 cm. $\times$ 1 cm. I suggest that makers increase the intensity of the field at least 10 times; preferably more.

The bar magnet is now *rigidly secured* to the observation table and the case of the standard compass turned until the reading of the dial corresponds exactly to the reading "true north," as deduced from the certificate for the compass.

On a wall at as great a distance as possible a bright vertical line is set up in such a position that the sighting line bisects it. For convenience the bright line should be only slightly broader (apparently) than the sighting line. The vertical plane passing through this line and the centre of the observation table may now be taken as the magnetic meridian for the purpose of testing compasses, and no appreciable error will result. For instance, if the intensity of the new magnetic field is only 10 times that of the usual horizontal field, and if the daily change in direction of the latter (in the particular workshop) is 2° , then the maximum error due to the Assumption of constancy of direction of the field where the compass is situated is two tenths of a degree. If the bright vertical line which is sighted through the slit is 114 cm. away, then at this distance, 2 cm. measured horizontally, represents 1° . It is wise, therefore, to place the bright vertical line on a distant wall. If arrangements for turning exactly through 90° , 180° and 270° are added, errors of bearing for N., S., E., and W. settings may be determined.

The arrangement described costs but a few shillings to instal and is absolutely reliable. It will be seen that the testing of compasses does not involve the use of a non-magnetic hut if a reliable standard compass is available. The latter need not be specially made; a good prismatic compass with negligible friction error is suitable. It should be standardised at some such

laboratory as the National Physical Laboratory—where a non-magnetic hut is essential—and re-standardised from time to time in case of change. The arrangement described must not be used to take the normal period of the magnet system, nor the error due to friction between pivot and jewel.

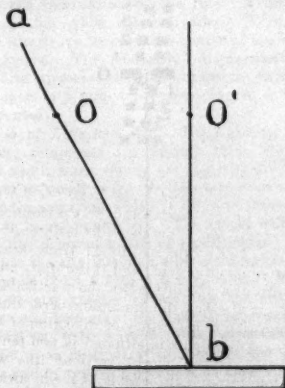


Fig. 6.

Possible Error No. (4).

With regard to the divisions on the dial, most engraving machines are so accurate that the error is generally too small to be considered. I have no knowledge of a prismatic compass being rejected owing to errors due to the dividing of the dial, but in hand painted cards for ships compasses the error may be considerable.

Possible Error No. (5).

The want of verticality of the sighting line may produce appreciable error. In fig. 6 if the

compass is placed on a horizontal surface and $a b$ is the sighting line, it is the lower end b which apparently intersects the scale.

The direction of the object O which the line is made to cover is read off as though it occupied the position O' . The error due to the line being



Fig. 7.

out of the vertical is not constant but is dependent on the angular altitude of the object under observation. It is not uncommon for the error due to the line to be as great as half a degree.

When testing a compass for the magnitude of this error the instrument is placed on a carefully levelled horizontal table and turned until

the lower end of the line apparently bisects the white space between two lower blackened squares (see fig. 7) on a vertical board situated 228 cm. from it. The upper part of the line when viewed now appears to bisect the white space between two blackened squares on the upper part of the board, and the number corresponding to these squares gives the possible error due to the inclination of the line. From the centre of the white space (reading 0) a silk plumb line drops through the centre of the lower white space. The horizontal displacement of the white spaces between successive pairs of black spots is 4 mm. (corresponding to a tenth of a degree).

Width of Line and Slit.—The sighting lines in some compasses are unduly thin, the apparent width when viewed through the slit being at times as small as $5'$. In the majority of instances the apparent width is $15'$, and this width is one to be recommended. The actual width of such a line is about a fifth of a millimeter. With regard to the slit this may with considerable advantage be from 0.3 to 0.4 mm. wide. Manufacturers err greatly in making the slits too narrow, and because of the difficulties in making observations with narrow slits, especially at twilight, it is customary to measure the width of slits and to regard a compass as unserviceable if the width is less than a fifth of a millimeter.

Possible Error No. (6).

The prismatic effect of the glasses of the window and cover is very rarely serious and the error introduced is generally of the order of 0.1° . Most pieces of glass having flat surfaces (unless specially worked) are slightly prismatic, and some makers test the glasses before marking the line on the window. The glass is turned so that any displacement of a ray of light is in a vertical plane: if so used in the window no prismatic error is introduced.

Possible Error No. (7).

The magnetic effect of the case and metal fittings is important. Some cases are cast, others stamped, while others are spun. If a case is slightly but uniformly magnetic there is no error due to it, but if particles of iron or steel are present, thus destroying symmetry, an error varying with the position of the magnet is introduced. Most brass screws are magnetic; some of them are appreciably so, and their introduction into a case may produce errors as large as 6° . As most brass intended for the manufacture of screws has a considerable content of iron (I am told that it is added to make the machining easier) great care should be exercised in choosing purchased screws. Some makers turn their own screws from brass rod. The hinge is frequently faulty. In addition I have found in a few instances that the thin brass strip used for the lifter, stop, and lubber's mark is appreciably magnetic.

Errors due to part of the case or screws being of magnetisable metal cannot be quantitatively determined without the aid of a special instrument. It is very easy by means of a small delicately poised magnet to show that many parts of an ordinary compass case are of magnetic material, but to reject a compass on the evidence of such a test would be most unfair to the maker and very unwise on the part of a testing department. In many instruments I have been able to prove that the screws in the prism boxes are appreciably magnetic, but their effect on the compass needle was so small that I failed to detect it. If a particular portion of a compass case, say the hinge, be of magnetic material, its effect on the reading of the compass depends on the intensity of the magnetic field (due to the needle) in which the hinge is situated, and also on the distance of the hinge from the needle. The stronger the magnet, the greater the effect; the greater the distance, the less the effect. With

increasing distance the effect falls off very rapidly.

If the compass needle were supported on an absolutely frictionless pivot then the magnet would always point due magnetic north, conditionally that no magnetic material was near it.

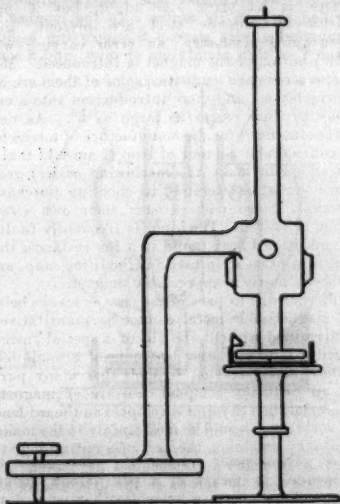


Fig. 8.

The rotation of the case, if non-magnetic, would not affect such a needle. While such a test is impossible owing to the friction between the pivot and jewel, it is easy to design an instrument for testing a compass case on this plan. A sketch of the instrument is given in fig. 8. The

essential parts consist of a magnet of the same length as the magnet of a prismatic compass and of shape shown in figs. 8 (a) and 8 (b).

The moment of this magnet should be about 40 C.G.S. units, this being an average value for the needles of most Mark VII compasses. The magnet has a circular hole 1 cm. in diameter drilled through its centre. To the magnet is connected a wire stirrup S supporting a fine vane V of thin aluminium or mica, and a con-

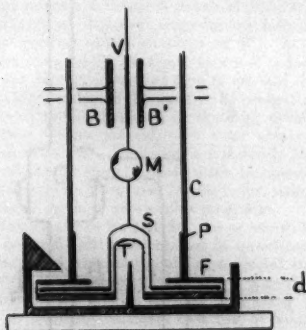


Fig. 8 (a).

case galvanometer mirror M. The whole is suspended by means of a silk thread about 10 cm. long. D is a brass disc having a diameter about 0.5 mm. greater than the length of the magnet. A thimble T about 8 mm. diameter passes through the hole in the magnet and the spring pieces P P grip the supporting tube C and hold the disc in position. The outside distance d is small, not more than 5 mm. To prevent draughts affecting the motion of the needle the disc D and the flange F are in contact except for a space milled out of D in which the magnet

swings. The result of this milling is to leave two small rectangular apertures between D and F which I have generally closed by means of paper or mica.

The vane V is a damping vane 3 cm. $\times$ 1.5 cm. and the buffers B B' are moved inwards to damp the motion of the system and so prevent undue oscillation.

The arrangement so far described has for its object the replacement of the magnet of a pris-

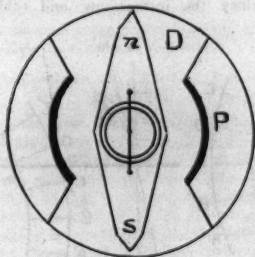


Fig. 8 (b).

matic compass by a magnet of the same length and practically the same moment but whose motion is not controlled by solid friction.

To test the case of a compass and to measure in degrees the error which is likely to be produced by magnetic material in the case, the cover glass and dial must be removed. The case, with the window vertical or inclined, is then raised to the correct height. The point passes into the thimble T, and the prism box is raised and pushed over as when making observations. The case is then *very slowly* rotated. By means of the mirror the image of a bright filament is thrown on a scale divided into degrees, and by observing the motion of the

image the error due to the case can be directly read off.

There are several details I need not describe. The general principles will readily be grasped. I need only add that the table must have a centering device in order to centre the compasses quickly and accurately. Also a stop should be added in order not to raise a case too high. Little conveniences like a grooved spindle to prevent the table falling if the hand is removed should also be added. At the National Physical Laboratory the instruments and tables are

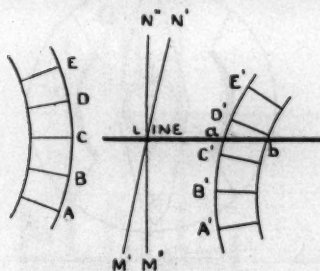


Fig. 9.

bolted to rigid slabs to prevent relative displacement—a very necessary precaution. In all I have set up five of these instruments, and the results obtained with them are very satisfactory. The time occupied in testing a case is about two minutes.

Possible Error No. (8).

Not unfrequently errors of bearing are produced through faulty setting of the prism in the prism box. It is necessary that the vertical plane containing the slit and line should be normal to the reflecting surface of the prism. When this is not so the graduations on the dial appear

to be non-radial, and the appearance of the line and dial is somewhat as shown in fig. 9. I have examined compasses in which the difference between readings at a and b amounts to half a degree. But the error due to the non-symmetry of the prism with respect to the radius of the dial does not lie entirely in the uncertainty of this reading. This will be apparent from fig. 9, where A B C D E represent degree divisions on the dial, $M'N'$ the correct, and $M'N$ the actual, position of the base of the prism, and A'B'C'D'E' the position of the image.

Care in machining and assembling the parts is necessary to eliminate this defect.

Possible Error No. (9)—Friction Between Pivot and Jewel.

The commonest defect of all is excessive friction between pivot and jewel. This produces an uncertainty in the readings, which invariably increases with prolonged use of the compass. The study of this frictional error necessitates an examination of all of the forces acting on the magnet system when it is oscillating.

When the magnet is moving away from its position of equilibrium there are four distinct forces acting on the moving system, all of which tend to stop its motion. These forces are:—

(1) Magnetic force due to action of earth's magnetic field on the magnet.

(2) Fluid frictional force due to retarding action of the medium in which the system swings. In the case of an ordinary prismatic compass the medium is air. In liquid compasses the medium is a mixture of spirits and water, or of glycerine and water.

(3) Electromagnetic retarding force due to the production of electrical eddy currents in the metal case.

(4) Solid frictional force due to the friction between pivot and jewel.

Forces (2) and (3) depend on the velocity of the moving system. When the velocity is great

these forces are of considerable magnitude. For small velocities they are negligible. The meaning of great and small velocities will presently be seen.

Force No. (1) is proportional to the displacement of the moving system, but force No. (4) is independent of the velocity and of the displacement.

Forces Nos. (1), (2) and (3) are beneficial to the action of a compass. Force No. (4) is not.

The important thing to observe in the manufacture of a compass is to make force No. (1) large, i.e., the magnet should be strong, and to make the ratio of force (1) to force (4) as great as possible. In the case of the ordinary prismatic compass forces (2) and (3) are considerable, but it would be of advantage if they were greater still. The greater they are the sooner will the system come to rest, and this without any sacrifice of accuracy.

Mathematical expressions connecting these forces are not very helpful without experimental data, and I have attempted, therefore, to obtain the relative magnitude of the forces in a few compasses by direct experiment. I shall deal with three compasses only: (a) a very good compass with negligible frictional error, (b) a rejected compass with considerable frictional error, (c) a compass which just passed the test for friction.

With regard to force No. (1) (the magnetic restoring force), this can readily be calculated for small displacements from the well-known expression $MH\theta$, where M is the moment of the magnet, H the horizontal intensity of the earth's magnetic field, and θ the angular displacement from the position of equilibrium. The method for finding the moment of the magnet will be described later.

It is clear that since force (2) is due to friction between the moving system and the medium in which it swings, we can abolish it by removing the medium. That is, let the system swing

in vacuo. Also, since force (3) is due to electrical eddy currents produced by the moving mag-

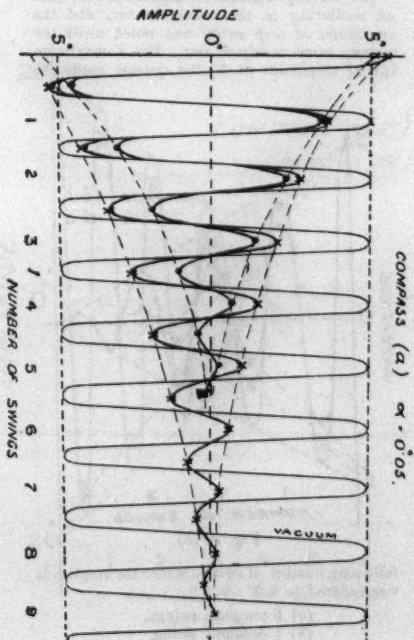


Fig. 10 (a).

netic field cutting the metal case, it can be abolished by removing the case.

I now give the results of the experiments on the three compasses (a), (b) and (c).

The moving systems of the compasses were set oscillating in the usual manner, and the amplitude of each swing was noted until the systems came nearly to rest. For a maximum initial amplitude of 5° the systems made the

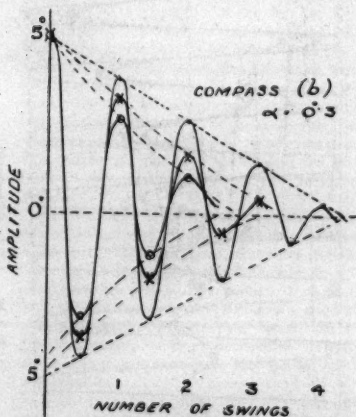


Fig. 10 (b).

following number of swings before the amplitude was reduced to 0.2° :—

- (a) 6 complete swings,
- (b) 1 complete swing,
- (c) 2 complete swings

The amplitude and numbers of swings have been plotted. The results are shown in figs. 10 (a), 10 (b) and 10 (c).

Observations with the magnet supported in

air and inside the metal case are indicated thus $\odot$ in the figures.

The pivots were now removed from the cases

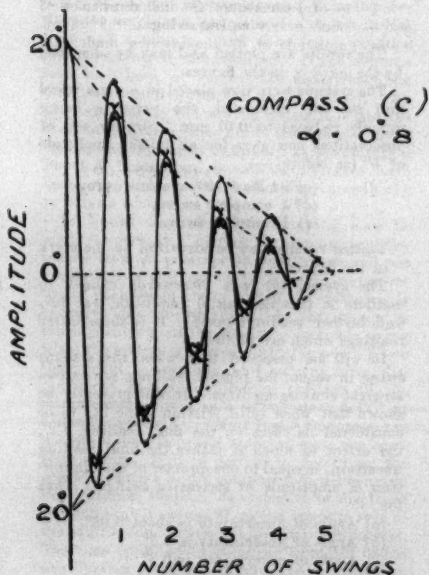


Fig. 10 (c).

and secured to stiff cardboard. A shallow cardboard cylinder was substituted for the case and the top covered with mica. In this way the electromagnetic retarding force was removed.

Observations similar to the preceding ones were now made, the number of complete swings being :

- (a) 10 complete swings,
- (b) 1 complete swing,
- (c) sometimes 2 and sometimes 3 complete swings.

The results are plotted and may be identified by the mark $\times$ in the figures.

The systems were now placed in a glass vessel and the air exhausted, the pressure being quickly reduced to 0.01 mm. Similar sets of observations now gave for an initial amplitude of 5° (as before) :—

- (a) At least 100 complete swings.
- (b) 1 complete swing,
- (c) 4 complete swings.

Plotted results may be identified by the mark in the figures.

The system (b) was afterwards caused to oscillate so that the initial amplitude was 20° , and further readings taken. It is these latter readings which are plotted.

It will be observed that when the systems swing in vacuo, the plotted readings are on two straight converging lines. It will presently be shown that when solid friction alone has to be considered its effect on the final reading, *i.e.*, the extent to which it makes the final reading uncertain, is equal to one quarter of the diminution of amplitude of successive swings. Thus for

- (a) Angle of uncertainty is about 0.05° .
- (b) Angle of uncertainty is 0.8° .
- (c) Angle of uncertainty is 0.3° .

Of course, compasses are not tested in this way to determine the angle of uncertainty, or error due to solid friction. The method used will be described later.

It will be seen that the rapid reduction or "damping" of the oscillations caused by air friction and electromagnetic action is very con-

siderable. In the instance of compass (a) an oscillation of 5° amplitude (10° of swing) would, after one minute (the period was 4.5 seconds), be reduced to 4.3° by solid friction only, but the air friction and electromagnetic damping reduce the amplitude to a fifth of a degree in less than half a minute. And these forces produce no uncertainty in the readings. If the compass case had been of copper the electromagnetic damping would have been much greater. By properly shaping the moving system it is possible also to increase the fluid friction. Indeed during the testing of compasses at the National Physical Laboratory numerous instances have been met of marked fluid damping in ordinary prismatic compasses.

In liquid compasses this damping may be made "aperiodic," so that the system when displaced from its position of equilibrium will not oscillate, but will return in a single movement to its position of rest. When the damping is excessive the time occupied in the return is much greater than the time of a complete swing when the system is only slightly damped. When the system is aperiodic and returns after displacement to its equilibrium position in the minimum time, it is said to be "dead beat" or critically aperiodic. Most liquid compasses are not quite dead beat but make at least one complete swing. If it is desired to determine the period of such a liquid compass difficulties arise. For instance, suppose the system to be displaced 10° and that it swings 2° on the opposite side of the equilibrium position before it comes to rest. The time occupied in swinging from 10° to the equilibrium position is different from that taken from the latter position to a displacement of 2° in the opposite direction. The half-period may be taken as the time occupied in passing from the 10° displacement to that of 2° , but it is well known that the errors of such a measurement are considerable. If the period is desired it is best to displace the system, say 10° , by a magnetic

field due to an electric current. Then switch off the current and note the time T taken by the system to first pass its position of equilibrium, and note the maximum displacement on the opposite side. Then the half period is given by the expression.

$$T / \left(1 - \frac{\alpha}{180} \right) \text{ where } \cot \alpha = 1.5 (\log d_1 - \log d_2).$$

d_1 and d_2 are two successive displacements on opposite sides of the equilibrium position.

I proceed now to indicate the theory by which it is concluded that if an oscillating system is affected by solid friction only, the angle of un-

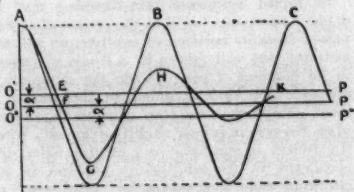


Fig. 11.

certainty is given by one-quarter of the diminution of amplitude of successive swings.

When there is no solid or fluid friction and no electromagnetic forces tending to stop the oscillations, the time of swing of the system is given by the equation

$$T = 2\pi \sqrt{\frac{I}{MH}}$$

where T is the time in seconds of a complete swing, I is the moment of inertia of the system, M is the magnetic moment, and H the

horizontal intensity of the earth's magnetic field. In England the value of H may be taken as 0.18 C.G.S. unit. Such a system will oscillate for ever and the oscillations may be represented by a curve $A B C$, etc. (Fig. 11.)

Let there be now a retarding couple due to solid friction. According to our knowledge this solid friction is independent of the velocity. Let the restoring couple due to solid friction be D . When the magnet is displaced through a small angle θ from its position of equilibrium, the restoring couple due to the action of the earth's magnetic field is $M H \theta$. The ordinates of fig. 11 may, therefore, be taken as measures of the amplitude and also of the magnetic restoring couple. The dotted lines $O'P'$ and $O''P''$ are situated at a distance a from $O P$ such that $M H a = D$. When the system moves outwards from O to A the total retarding couple is $M H \theta + D$, and varies continuously. When moving inwards from A to E the accelerating couple is $M H \theta - D$, and is a maximum at A and zero at E . After passing E the couple changes sign, i.e., D is greater than $M H \theta$. The passage from E to F is, therefore, dependent on the momentum of the system, and it is apparent that as the oscillations diminish in amplitude the system may come to rest anywhere between the lines $O'P'$ and $O''P''$. The rate of diminution of amplitude is easily found. Thus, when the system moves from A to G the accelerating couple at any point on $A G$ at a distance θ from $O P$ is $M H \theta - D$, the sign of θ being changed on crossing $O P$. This accelerating couple may be written as $M H (\theta - a)$. If, therefore, we reckon angular displacements from the line $O'P'$ we get symmetry of the curve $A G$ about this line. Hence it follows that the part of the curve $A E$ is similar to the part $E G$. Also the time taken for the system to move from A to E is identical with that taken for it to pass from E to G . The decrease in amplitude in the half period is therefore $2 a$, and on each complete

swing there is a loss in amplitude of 4α . The form of curve resulting is shown in fig. 11. (Curve A H K.)

The angle α is a measure of the frictional error, and is called the angle of uncertainty. If the system be set oscillating the final reading for bearing is uncertain, because of solid friction, by the amount α .

It is apparent that if in a prismatic compass there is negligible fluid friction and negligible electromagnetic opposing forces, the solid frictional error is readily measured by reading successive amplitudes in the same direction and dividing the difference between them by 4. But we have seen that in prismatic compasses the fluid frictional force and the electro-magnetic opposing force are by no means negligible, and because of these forces the oscillating system comes to rest much sooner than it otherwise would.

It is evident that in practice the oscillating system will come to rest with an error averaging about $\frac{1}{2}\alpha$, and if, therefore, a system is set oscillating a number of times in such a manner that the initial displacements are equal but opposite in direction for successive experiments, the average difference between two successive readings may be taken as a fair measure of the frictional error. Such a procedure is adopted at the National Physical Laboratory. Initial displacements of 5° are produced by passing an electric current through a coil of wire about 20 cm. away from the compass. Reversals of the displacement are produced by reversing the current. In general not more than eight readings in all are taken for at least two positions of the compass. The mean difference between the rest points is taken as the value of α , the frictional error or angle of uncertainty.

In a few rejected compasses the frictional error has been greater than 10° . In the majority of instances, whenever a frictional error exceeds half a degree the value obtained varies con-

siderably with change in the relative position of pivot and jewel. In certain positions the frictional error is often very excessive. This is no doubt due to a want of symmetry of the point and jewel about the axis. In such cases it is practically impossible to repeat the readings, but it is not difficult to confirm the fact that the friction is excessive.

Having explained the cause of the uncertainty the remedy must be sought. This, briefly, consists in improving the pivot and the jewel, in increasing the intensity of magnetisation of the magnet, and in reducing the ratio of the moment of inertia of the moving system, to the moment of the magnet.



Fig. 12.

I have examined under the microscope a large number of hard steel pivots in compasses from various makers. Many of the conical points showed flat reflecting surfaces, and in a few instances the pivots were clearly too soft. The general shape of the point was usually correct. The pivots should be of hard steel, but in general not quite dead hard. A good shape (copied from a photograph) is shown in fig. 12. The points should be ground in the lathe, and on completion should have a good polish. Gramophone needles are exceptionally well finished, but the points are too flat for use in compasses. Some compass pivots are ragged; under the microscope

a "rasp" appearance is at times observed. More friction is then produced by the jewel rotating in one direction than in the other.

With regard to jewels, agates are usually employed but garnets are better. Agates vary considerably in hardness, and care should be taken to choose hard stones. The polish is important. Under the microscope a well finished jewel is transparent; I have examined many which have acted as small lenses. In other instances visible wear in soft agates has been observed, and in two cases it was possible to photograph the surfaces. The photographs revealed conical pits in the agates.

In all instances it may safely be asserted that any error due to friction between pivot and jewel will increase with the use of a compass. Good pivots and good jewels are therefore of fundamental importance. The very best type of pivot is a short hard steel pin forced into a larger brass pin and ground up in the lathe. A dead hard point can then be used. In liquid compasses the points should be of iridium.

The Magnet and Dial.—It will be observed that in the list of errors (p. 2) no mention is made of the strength of the magnet or of the time of swing. Both of these are, however, measured in testing a compass.

The time of swing is, of course, directly related to the strength of the magnet, and the latter also controls the angle of uncertainty, or frictional error. For a given size of magnet a small magnetic moment indicates either bad steel, insufficient hardening, or that the magnet has been magnetised in too weak a magnetic field. I find that insufficient hardening and magnetisation in a weak field are the principal causes of weak magnets. In the past many manufacturers have applied to the National Physical Laboratory for information on the best way of magnetising the steel pieces. A cheap and efficient way is to use a magnetising coil, the current through which enables the necessary field

strength to be obtained. In a footnote* I give particulars of such a coil. By means of this apparatus a steel piece is magnetised to the maximum extent in a few seconds.

I find from experiments that a magnet 45 mm. long and 0.8 mm. thick, such as is used in many prismatic compasses, can be magnetised to have a moment of about 80 C.G.S. units. If 30 per cent. is deducted from this for ageing the moment is about 56 C.G.S. units. The pieces experimented with were hardened and tempered to a very light straw colour. Apart from risk of breaking, magnets are best dead hard. They were magnetised in a field of an intensity of about 400 C.G.S. units.

To measure the moment of a magnet, the instrument already described for testing the magnetic quality of cases may be used. In such a case, however, the damping vanes must be drawn well back to allow for a considerable angle of swing. This naturally reduces the damping

* The pieces of steel should be magnetised singly in a solenoid, the current through which produces a field inside the solenoid not less than 400 C.G.S. units. A suitable solenoid may be constructed by winding six layers of 120 turns per layer of No. 18 double silk covered copper wire on a brass tube $2\frac{1}{2}$ cm. in diameter and 15 cm. long. Two soft iron blocks 1 cm. $\times$ 1 cm. $\times$ 4.5 cm. are fixed 6 cm. apart into a wooden frame, which can slide into and out of the coil. The central part of the sliding frame carries a steel pin on which the steel pieces (with jewels attached) can be magnetised. The height and position of the pin should be such that the axis of the steel piece to be magnetised practically coincides with the axis of the coil. The resistance of such a coil is about 1.1 ohms, and the current required to produce a satisfactory magnetising field is about 5 amperes. A 6-volt accumulator suffices therefore to supply the current.

very considerably. When time is very precious it is better to set up a simple magnetometer with a needle about 5 mm. long and a large damping vane, so as to get a well damped instrument with a long time of swing. A concave mirror is added as before. The suspension may be of silk, and a simple tube and base suffice for the parts of the instrument. The magnet is allowed to come to rest in the magnetic meridian, and the needle of the prismatic compass is placed in the end-on position of Gauss and 20 cm. distant. By means of a lamp and scale the resulting deflection in degrees is read off. It is better to obtain the change in deflection on reversal of the magnet. In the latter case the moment of the magnet is given by multiplying the tangent of the angular deflection of the system (half the angular deflection of the spot of light) by 360.

As already stated, the time of swing of the system of a prismatic compass is given by

$$T = 2\pi \sqrt{\frac{I}{MH}}$$

where T is in seconds, I is the moment of inertia of the system, M is the magnetic moment, and H the horizontal intensity of the earth's magnetic field. It is desirable that T should not exceed seven seconds. T may be reduced by reducing I , and this reduction will in general be accompanied by a reduction in the weight of the system.

The moment of inertia of the dial is given by

the expression $\frac{m r^2}{2}$ where m is the mass in

grams and r is the radius in centimeters. The moment of inertia of the magnet is dependent on its shape, but for such a magnet as is shown in fig. 4, a sufficiently

exact expression is $\frac{m_1 r^2}{4}$ where m_1 is the mass

of the magnet and r is half its length. The latter is in most cases practically equal to the radius of the dial. If the moment of inertia of the jewel support be neglected (it is always very small), the total moment of inertia of the swinging system may be written

$$\frac{r^2 (2m + m_1)}{4}$$

Measurements show that the better class mother of pearl dial is about 0.5 mm. thick and weighs 2.3 grams. The magnets vary, but are usually about two-thirds the weight of the dial. Reduction in the thickness of the dial is, therefore, the best way to reduce the moment of inertia. Usually the moment of inertia of the dial is three times that of the magnet.

On the whole I am inclined to think that with mother of pearl dials the magnets are preferably not less than 1 mm. thick. In such a case the total moment of inertia for a dial (0.5 mm. thick) and magnet is about 5 gm. cm. units.

The magnetic moment of such a magnet, if made of good steel, may, after ageing, exceed 50 C.G.S. units, and the time of a complete swing of the dial system will not exceed five seconds. When testing compasses at the National Physical Laboratory the moment of a magnet 4.5 cm. long is regarded as too small if it is less than 18 C.G.S. units, and a system is regarded as too sluggish if the period exceeds 7.0 seconds. It is clear that for such a weak magnet the moment of inertia must be correspondingly small for the compass to be satisfactory.

Discussion.

MR. HENRY A. HUGHES thought what they wanted to aim at in that discussion was to get as much practical experience as possible, and he would, therefore, make way as soon as possible for his brother, who, as works manager

of the firm to which he belonged, had had more practical experience in these matters than himself. Just one point he might, perhaps, be allowed to mention. He had so often heard that compasses tested by the manufacturer had come back from Teddington with different results, and he wondered whether that was not partly due to the pivot and the stone. If the jewel was a little soft, or the pivot not quite right, might not the usage it received before going through its second test affect the result? And was it not possible for a compass to pass a satisfactory test, and yet afterwards, in use, develop considerable error? He had had personal experience of one compass which had been sent out to the Front, and when it was returned after only a few months, there was an error of quite 3° in it; and it was a matter of interest to know how such errors crept in.

MR. A. J. HUGHES said there was one source of error which Mr. Smith had omitted to mention, namely, errors due to imperfect balance of the card. That might not be a very great point with prismatic compasses, but with any compass of larger diameter it certainly came in to a very great extent. Only that morning he had had under his notice a compass of $2\frac{1}{2}''$ diameter which showed an error of about 4° due to that want of balance. As to the question of the shape of the needle, a certain amount of experimenting had lately been done in that direction, and although the one given by Mr. Smith was admirable he would like to submit one which, though he would not say it was an improvement, might, at any rate, be thought worthy of consideration.



That would prove an even better needle, he ventured to think, than the one outlined by Mr.

Smith. The experiments made lately agreed almost exactly with those conducted by Captain Kater some years ago, and a needle of that shape would give an equal magnetic moment with less weight than the shape showed by Mr. Smith. That was surely an advantage, especially with a small compass like a prismatic; and it was being used also with larger compasses and found to be of great advantage. Mr. Smith had alluded to the loss of magnetisation of the needles. He had with him some little sketches of needles just about the size of a Mark VII. needle which had been magnetised, and one of them had an irregular magnetisation, as could be seen from the iron filings. These little papers were done to discover what was the effect of softening the points and softening the centre, and the results had greatly surprised himself and other practical compass makers, though he did not suppose they would surprise Mr. Smith. (Laughter.) Compass makers had always imagined that the point should be dead hard, but, as a matter of fact, these little diagrams showed that it was almost immaterial if the points were slightly softened, while, on the other hand, if the centre was softened it was a very serious matter. He did not suggest that either should be softened, but wished merely to point out the essential fact that the centre should be dead hard. He did not quite understand why Mr. Smith should have laid such stress on variation in magnetic condition due to the want of a non-magnetic hut. He did not know what the figures were, but the diurnal variation was comparatively small, and he could not understand why that diurnal variation should make so much difference in the ordinary compass maker's premises. They certainly did not find that difficulty in testing prismatic compasses. With regard to fluid friction, he wished to point out that this was not always favourable, as he rather inferred was Mr. Smith's opinion. A case might arise in which the fluid friction

would be acting with a known error of the compass. He did not say that that had any bearing on prismatic compasses, but he mentioned the matter because he had happened to come across a case quite recently in which in one half-turn the fluid friction was against the error and in the other half-turn was contributing to it. In conclusion, he thought, the points brought before them by Mr. Smith, and the evidence he had shown of the extremely thorough way in which these questions were being dealt with at Teddington, would prove exceedingly valuable to compass makers in general. (Applause.)

MR. PERKINS remarked that Mr. Smith had stated that if he had to make compass needles he would have them stamped with a centre hole and two smaller holes, one on either side. Judging from some of the stampings he had seen—and they were done by a very able firm—it appeared that the makers could not stamp even one hole centrally—(laughter)—so he did not know how they would get on if they had to stamp three. Then again, a further trouble was to get the dial absolutely centred.

LIEUT.-COL. GIFFORD said he had been greatly interested by Mr. Smith's allusion to the influence of the brass screws. He would like to ask whether Mr. Smith attributed that influence mainly to magnetic impurities in the metal of which the screws were made, or to the action of the taps or dies with which the screws had been operated on. It was, of course, a well known fact that when a hard metal and a less hard metal were brought in contact with one another, minute particles of the harder metal embedded themselves in the less hard metal. Was the influence referred to attributable to this factor, or was it due to impurities in the metal itself?

DR. G. B. BRYAN said there were two questions which he would like to ask Mr. Smith. The first was whether there was any use in tapping the compass before using it. It was usual, in his

experience, in physical laboratories to tap pivotal instruments of any kind. Was there any value in this, or was one apt to tap it into a wrong position? The second question was this: Mr. Smith had referred to a method of magnetising so as to get the magnetic axis in coincidence with the geometrical axis, but he had not told them how to do this accurately. If he would kindly do so, he thought it might be of some advantage.

MR. S. D. CHALMERS pointed out that if the errors of a compass be recorded for each of the four points, N., E., S. and W., valuable information is given not only with regard to the actual adjustments but to the possibility of adjustment. The following example from an actual compass illustrates this point:—

Readings	N	-0°.2
	E	+0°.8
	S	-0°.4
	W	-0°.0

The mean of the four readings +0°.05 indicates the needle is correctly placed on the dial as regards direction.

$N - S = + 0°.2$ gives the eccentricity error due to displacement of the needle across the dial. In this case the centre of the dial does not coincide with the pivot.

$E - W = + 0°.8$ gives the eccentricity error in the direction at right angles.

$$\text{But } \frac{N + S}{2} = - 0°.3 \text{ and } \frac{E + W}{2} = + 0°.4$$

should give the same value, and any difference is due to a compass which is to that extent defective, and the difference - 0°.7 is in a sense a measure of the goodness of the compass as distinguished from the accuracy of the adjustment.

THE PRESIDENT, after moving a vote of thanks (which was unanimously accorded), said that he was sure they would all agree that Mr.

Smith's paper was the right paper by the right man, at the right time. (Hear, hear.) That was the kind of paper they wanted in that society; and which they must have during the forthcoming year; if they got only a few of such papers that society, at any rate, would have justified its existence and "done its bit." (Hear, hear.) He had been rather amused when Mr. Smith pointed out the fact that the magnetic axis of the magnet might be displaced parallel to the 180 line of the disc without introducing any error whatever. Those among them who had attended science and art classes many years ago had very good reason for remembering that fact, because there used to be one favourite question given year after year which must have floored thousands of candidates, and which depended upon that fact. The question used to be given in a very simple way—a simplicity which often conduced to the fall. (Laughter.) "A lath six feet long with a magnet fixed transversely to it at one end is balanced upon the bottom of an upturned Florence flask: How will the lath set?" Well, of course, the true answer was east and west, but that was a fact which would only be driven with great difficulty into the heads of elementary students, and it was perfectly amazing how crop after crop of those elementary students were bowled out by that very simple question. One of the most valuable practical points in Mr. Smith's paper was to show in what a very simple way a practically non-magnetic hut could be obtained. He looked upon that as an exceedingly valuable part of the paper, because there was no doubt whatever that in most workshops, at any rate, the direction of the resultant field—he would not call it the earth's field—did vary considerably from time to time during the day. A friend of his had a very delicate magnetometer which he used to work with during the evenings, while engaged in research work. One morning he (the President) had met him in a rage,

and upon his being asked what was the matter, he said: "The whole of last night was wasted, because the silly ass next door never put the poker down twice in the same place." (Loud laughter.) Another important point in the paper was this. Mr. Smith had told them that the magnet should be made with three holes in it. He did not know whether it was a fact, but he thought it quite possible that the Government specification provided only two. Well, he might tell them at once that the Government Department concerned did not think finality had been reached in these matters—(hear hear)—and that if anyone concerned in the production of prismatic compasses had actually got some practical idea for simplifying their manufacture and increasing their production, all they had to do was to propose that they should be allowed to adopt that modification, and send in their proposal to the Ministry of Munitions (Designs), and any proposal which any practical man sent along for the modification of any detail, no matter how small, in any optical instrument manufactured at the present time, would receive the utmost consideration; it would be treated seriously and, if the conclusion arrived at was that it would simplify manufacture or increase production without loss of efficiency, they might be satisfied that it would be adopted. He might say, also, that the Council, sitting before that meeting with a copy of Mr. Smith's paper before it, had come to the conclusion that it was a very valuable paper, and one which should be made accessible to the trade at the earliest possible moment. The result was that that paper would be published in pamphlet form—possibly a small charge being made for it—at the earliest possible date, so that any who were interested would be able to procure it and study it at leisure. Some of the speakers, in the course of the discussion, had added very valuable matter to the substance of the paper by means of their criticisms, and the Council would be very pleased indeed

if any who had spoken would send in an abstract of what they had said, otherwise the abstract would probably be of too brief a nature to be of the utmost value. He might perhaps refer, amongst others, to the addition to the discussion made by Mr. A. J. Hughes, which, he thought, ought to be published as far as possible *in extenso*.

Reply.

MR. F. E. SMITH, in reply, said it was quite true, as Mr. A. J. Hughes had observed, that a compass might be tested and passed, and, after two or three months' use, found to be not very serviceable. Much depended on the user. No doubt, if a thorough and complete test of a prismatic compass were to be made, it would be necessary to subject the jewel and the pivot to some form of vibration test and afterwards see whether the pivot or the jewel had been appreciably worn, but, he was afraid, this would mean great expense and considerable complication.

With regard to the imperfect balance of the card, which Mr. A. J. Hughes had referred to, he might say that that was incidentally tested, because, when the errors of bearings were taken, the prism was focused on the card for N. position; then the observer swung round to E., and came back again through S. and W. If he had to change the focus of the prism, the dial was out of balance, and if badly balanced the compass was rejected. Next with regard to want of balance due to the vertical force of the earth's field. In an ordinary prismatic compass the moment of the magnet was about 40 C.G.S. units; the maximum he had measured was about 70 units and the minimum about 8 units. If one calculates the want of balance due to the earth's field in an average compass it is equivalent to placing a weight of less than 0.01 gram on the edge of the dial. Experiment shows that this alters the balance by a negligible amount. But in the case of a large compass,

like a ship's compass, one may conceivably get want of balance due to the effect under consideration. With regard to the shape of the needle, which Mr. Hughes had indicated, that particular shape had a small de-magnetising effect; the broader the poles the greater the de-magnetising effect. He found there was not much advantage in cutting down the weight of the needle, for the reason that the weight of the dial was considerably greater; the latter weighed in general from 2 to 3 grammes.

Most of the mother-of-pearl dials were $\frac{1}{2}$ mm. in thickness, and they were fairly uniform; he had calculated the moment of inertia of the dial and found it to be three times as great as that of the needle. He would suggest, if any change, slightly heavier magnets. He had started a series of experiments with a view to ascertaining the best shape of needle, but had to abandon them partly for want of time and partly because the results were somewhat erratic owing to different qualities of steel being used. The hole drilled in a middle of a magnet had an effect on the moment of the magnet. In some cases there was a loss of 30 per cent. due to the central hole. A question had been asked with regard to the magnetisation of the needles. He was sorry he had not had time to describe the method, but it would be found in the paper. One of the faults he had not mentioned was that many magnets were not sufficiently hard, and could be cut with a knife. They ought not to be glass-hard, but should only be tempered to the faintest straw-yellow colour. Magnets should be very hard in order to retain their magnetisation. A good needle when first magnetised had a moment of about 70 C.G.S. units, but a loss must be allowed for, due to ageing, of about 30 per cent.

Magnets retained a moment of about 50 C.G.S. units for many years. With regard to the daily variation of the magnetic field in certain workshops, he could only say that several

manufacturers had told him they had trouble owing to this cause. Some had actually observed a change of 3° . He could understand that some were very unfortunate, because a room in which he himself sometimes worked had a variation of as much as 3° , due to the man in the next room switching on a motor when he ought not to. (Laughter.) The fluid friction could, of course, be overdone. The instrument should never be over-damped. A common type of electrical instrument had a motion termed "dead beat." The moving parts of such instruments come dead back to rest without oscillating. That was the ideal movement for a compass. There was also this advantage, that if a series of small rapid disturbances acted on such a compass the compass itself would not show their existence; it would only give the mean disturbance which was exactly what was wanted, whether the compass was being used on a ship, on an aeroplane, or anywhere else. Plenty of fluid damping was wanted, and the fewer swings the needle made before coming to rest the better. With regard to the difficulty of stamping magnets, he was very glad Mr. Perkins had mentioned that, because it might be a real difficulty.

[Note added, March 23rd:—Since the meeting one manufacturer has informed me that he stamps all of his magnets, the central hole and small screw hole being stamped out at the same time.—F.E.S.]

He (Mr. Smith) had questioned a very large stamping firm in London, and they had come to the conclusion that this work could be properly done, but he quite recognised that in some cases there might be difficulty, and that it might be impossible to get perfection of shape. Colonel Gifford had referred to the question of brass screws. He agreed with Colonel Gifford about the skin of magnetisable material, but he did not think the errors in compasses were principally due to this fact. Dr. Bryan had

asked whether it was a right practice to tap an instrument before using it. The answer to that was in the affirmative. It could only have beneficial effects. When an instrument was tapped, it was given a chance to oscillate, and the forces were always favourable for the needle to get nearer the correct resting point.

Perhaps it was not generally realised that the forces operating in the outward and return swings were not algebraically the same. In the outward swing all four forces were acting in the same direction and the total force was their sum. In the return swing three forces were acting in one direction and one against that direction. Actually the time of the swing of the compass in passing outward to the extreme limit is not quite the same as in the return to the equilibrium position. He thought he had already answered Dr. Bryan's question with regard to the coincidence of the magnetic and geometrical axes. Mr. Chalmers had spoken of the bearing errors, and asked whether makers could not identify certain features. No doubt that was a valuable suggestion, but one had to remember eccentricity was not always either N. and S. or E. and W., but somewhere in between. Mr. Chalmers' suggestion should have further consideration and, no doubt, the result would be helpful to makers. He thought that when people trusted an ordinary compass within half a degree they were over confident. There were certain compasses the errors of which never exceeded half a degree. He thought there should be two classes of compasses with distinct marks so that when a compass was required for special military purposes, one might be certain of obtaining the superior and not the inferior one.

ON LABORATORY MEASUREMENTS FOR WORKSHOP PURPOSES.

By MR. S. D. CHALMERS, M.A., VICE-PRESIDENT.

Read 13th April, 1916.

ALTHOUGH many of the methods described in this paper have a wider application, they will be described in relation to the optical system of a typical prism binocular, and it will be understood that the spherical tools will always be available in pairs and that test plates will be used throughout. Thus it will be possible to use both the convex and concave surfaces for the measurement of curvature or to select whichever is the more suitable for our purpose. The tools will always be made to take a block of three or more lenses when the diameter and radius of curvature of the lenses permit, so that the tools will always have a relatively large diameter.

The optical system of each telescope consists of:

- (a) The objective, a pair of lenses cemented together.
- (b) The eye-piece, which is a cemented doublet and a single plano-convex lens.
- (c) A pair of right-angled totally reflecting prisms.

A typical specification of an o.g. would be diameter 26 mms.

$r_1 = 50$ mms. convex	} Dense Flint.
$r_2 = 28$ mms. concave	
cemented to	
$r_3 = 28$ mms. convex	} B. S. Crown;
$r_4 =$ plane, or nearly plane	

while for an eye-piece a typical specification is the eye lens of diameter 12 mms.

$r_1 = \text{plane}$
 $r_2 = 12.75 \text{ concave}$ } Dense Flint,
 cemented to
 $r_3 = 12.75 \text{ convex}$ Med. Barium Crown
 $r_4 = -15 \text{ convex}$

and a plano-concave field lens of diameter 20 mm. and $r = 16$ mm. of French plate. The measurements dealt with are the curvatures of the surfaces and tools; the equivalent focal lengths and back foci of the lenses, and the angles of the prisms.



FIG. 1.

Measurement of Curvature.—In general optical work an accurate spherometer is the most universal, and probably the most accurate, method for general use, but there are special cases when other methods are preferable.

Spherometer.—With the spherometer we measure the sagitta of a chord of the surface, and determine the radius by the relation

$$r = \frac{c^2}{2h} + \frac{h}{2}$$
 where h is the sagitta and c is half the chord (fig. 2).

In the best instruments h can be read to .001 mm. and c is measured to .01 mm.

The instrument is thus best adapted to the cases where an error in h affects the value of the radius more than an equal error in c , but both c and h have to be measured accurately. (Note 1.)

NOTE.—The formula $r = \frac{c^2 + h^2}{2h}$ enables us to

determine the relative importance of errors in



FIG. 2.

measuring c and h . If Δc and Δh be the errors in c and h and Δr the corresponding error in r we have

$$\Delta r = \frac{2ch\Delta c - (c^2 - h^2)\Delta h}{2h^2}$$

Hence equal errors in h and c are of equal importance in the value of r .

When

$$c^2 - h^2 = 2ch, \text{ or}$$

$$h = (\sqrt{2} - 1) c = .41c$$

$$\text{i.e., } h = .29r, \text{ or } c = .7r.$$

If h be greater than this value the importance of errors in h becomes relatively less until when

$$h = (\sqrt{1.01} - .1) c = .905c$$

$$\text{i.e., } h = .9r.$$

An error in h of ten times the error in c would only produce the same effect.

If h be smaller than the value .41c, the importance of errors in h increase, until for

$$h = (\sqrt{101} - 10) c = .05c$$

the error in c may be ten times that of h ; for still smaller values of h the value of h is even more important.

There are two cases where the spherometer is obviously not the most suitable instrument:

(a) When the surface is so nearly plane that the curvature can be estimated by the Newton's Rings, which it gives when placed in contact with a true plane;

(b) When the surface is so nearly hemispherical that the maximum diameter can be taken to be twice the radius of curvature.

There is also the case where, owing to the small diameter of the lens, it is preferable to use an optical method of measuring the curvature.

Each of these methods can be extended to cover other cases, and may be made to cover all the typical cases for a prism binocular, provided advantage be taken of the fact that both convex and concave surfaces are available.

(1.) Measuring Microscope Method of Measuring Curvature.

(Applicable to a convex tool or surface whether polished or not, and suitable for the case when the depth exceeds 9/10ths of the diameter of the tool.)

Apparatus Required.—A measuring microscope capable of horizontal and vertical motions

and reading to .01 (or .02) mm. in each direction. The microscope must also be capable of a focusing adjustment once for all at right angles to these two motions.

An adapter into which the tool can be screwed is permanently fixed so that the vertex of the tool can be focused when the microscope is at the centre of its horizontal run.

In the actual apparatus a 2-inch o.g. with long working distance is used, and the total magnification is 20 times.



FIG. 3.

The general arrangement is shown diagrammatically in fig. 3.

Procedure.—The convex tool is screwed into the adapter, the microscope brought to the centre of its run and raised till the upper edge of the tool comes into the field; this edge is sharply focused. By using the two motions the microscope can then be made to follow the edge of the tool. The cross lines of the microscope are brought to the position (1) (fig. 4) on the surface, and readings V_1 and H_1 are made on the vertical and horizontal scales; the micro-

scope is moved horizontally till the cross lines come to the position (2), and the second reading H_2 is made on the horizontal scale; the mean of the readings H_1 and H_2 is taken, the microscope run back to this reading, and then raised till the cross lines are on the top edge

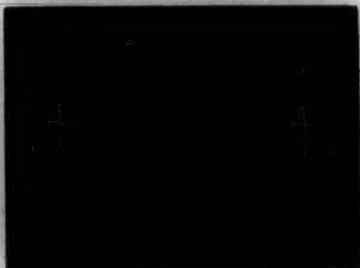


FIG. 4.

of the surface at (3) and the reading V_2 made on the vertical scale. Then

$$\begin{aligned} V_2 - V_1 &= A \\ H_2 - H_1 &= 2c \end{aligned}$$

and the radius is calculated by the spherometer formula $r = \frac{c^2}{2h} + \frac{h}{2}$

$$\text{meter formula } r = \frac{c^2}{2h} + \frac{h}{2}$$

In setting up care must be taken that the axis of the tool or lens is vertical; that the focus on the top edge is good, and that there is ample vertical movement for any tool that may be used. If the vertical movement be 10 cms. and the horizontal 20 cms. curves up to 10 cms. radius may be measured, but a larger o.g. would be required to clear the lens.

Example of measurement:
For convex tool.

$$\begin{array}{ll} H_1 = 5.876 \text{ cms.} & V_1 = 5.180 \text{ cms.} \\ H_2 = 10.740 \text{ cms.} & \end{array}$$

$$\begin{array}{ll} \text{Mean} & 8.308 \text{ cms.} & V_2 = 6.929 \text{ cms.} \\ & c = 2.432 \text{ cms.} & h = 1.479 \text{ cms.} \end{array}$$

$$r = \frac{c^2}{2h} + \frac{h}{2} = 3.566 \text{ cms.}$$

Optical Method.—Suitable for curves of short radius, say, up to 25 mms., and for test plates up to about 50 mms.

This method was described in its application to single surfaces before the Optical Society*, but when test plates are available the accuracy is increased, the calculations are simplified, and the range extended to include curves of 50 mm. radius.

Apparatus Required.—A measuring microscope capable of reading in one direction only, having a good focusing range, good cross lines in the eye-piece, and carrying a special target. This target consists of two parallel black lines, about 10 mms. long and $\frac{1}{2}$ mm. wide and separated about 200 mms. These lines, which are black on a white ground, are carried on a piece of metal attached to the microscope tube, so that the lines are symmetrical with regard to the tube (fig. 5). The line AB joining the centres of the two target lines passes through the axis of the microscope, and is parallel to the motion of the microscope.

Procedure.—One test plate is placed on the stage, and the image of one target line is sharply focused, the lens adjusted till the line comes central; the microscope stage is moved to bring the object on the cross lines, and a reading R_1 is made. The other line is then brought

* Transactions of the Optical Society, 1914.

on to the cross lines, and a second reading R_2 is made. The difference d_1 of the readings is taken.

The second test plate is measured in the same way, and the difference d_2 is obtained. Then $r = (d_1 + d_2)$ multiplied by the constant of the instrument.

The exact value of the constant depends on the separation of the two lines and their distance from the focus of the microscope.



FIG. 5. A—Attachment. B—Test Surface. C—Moving Stage.

In one actual instrument used the separation of the targets was 204 mms., the o.g. a 2-inch with a long working distance, and the tube length was adjusted to give a constant 1.25.

The simplest method of calibrating is to take a known pair of test plates and measure the values d_1 and d_2 .

As an example:

Convex test plate.	Concave test plate.	$d_1 + d_2$	r	Const.
d_1 21.20	d_2 17.74	88.94	49.79	1.2785

Using this value of the constant, other readings were made:

d_1	d_2	$d_1 + d_2$	r cal.	r known
19.63 ...	16.63 ...	36.26 ...	47.356 ...	47.36
10.20 ...	9.30 ...	19.50 ...	24.93 ...	24.94
4.04 ...	3.89 ...	7.93 ...	10.14 ...	10.14

In order to adjust to a constant 1.25, a sphere of radius 25.0 would give $d_1 = 10.46$ on the convex and $d_2 = 9.54$ on the concave.

The measurement may be carried out on any good measuring microscope or microscope with reading mechanical stage, provided the light from the target is not cut off by the tube or its support.

The author has used a Beck microscope with a mechanical stage, a Wilson measuring microscope, and the microscope which is used as the Zeiss Fokometer.

The method is applicable to small brass tools if these be polished sufficiently to give a good reflection.

(III.) Shallow Curves—Newton's Rings Method.

This method is applicable to shallow curves—i.e., with a radius above 500 mms.—and for measuring small differences of curvature.

The convex test plate is used and a measuring microscope provided with a vertical illuminator, such as is used for microscopic work on metals. This illuminator consists of a piece of plane parallel glass mounted in a short tube between the o.g. and the nose of the microscope and at 45° to the axis of the microscope. Yellow (sodium) light produced by burning salt in a Bunsen flame is allowed to fall on the reflector and is reflected through the o.g. (fig. 6).

The test plate is placed on a plane plate so that the point of contact is on the axis of the microscope. (The point of contact can be observed by looking obliquely below the microscope at the interference rings formed by the yellow light.)

The microscope is then focused on the ring system and the diameter of the first and any other convenient ring measured. It saves calculation to take the 18th or 37th ring.

Let c_1 be the radius of the first ring and c_2 of the 18th ring, then the radius is given by

$$r = \frac{c_2^2 - c_1^2}{17 \times .0005894} \text{ mm.}$$

which is within 2 parts in 1,000, equivalent to $r = 100 \times (c_2^2 - c_1^2)$ mms.



FIG. 6. Diagram of Arrangement. A—Reflector. B—Source of Light. C—Test Surface.

The illuminator was arranged so that the light was incident in the direction of travel of the microscope, and a 1-inch o.g. was used.

This method is specially adapted to measuring small differences of curvature.

Example.—It was required to determine the radius of a pair of plates of about 49.8 mms. radius by the aid of a known pair of plates of radius 46.36 mms.

The convex plate of the 46.36 pair was placed

in contact with the concave plate of the 49.8 pair, and measurements were made as before.

$$2c_1 = 17.33 - 16.27 = 1.06. \quad c_1 = .53.$$

$$2c_2 = 19.445 - 14.137 = 5.307. \quad c_2 = 2.654.$$

allowing the difference of curvature $1/r_1 - 1/r_2$ to be calculated as .001477, giving $r_2 = 49.79$ mm.

The repetition of the measurement gave the same result correct to 1 part in 1,000.

The actual instrument used was a Cambridge Scientific Instrument Co. measuring microscope but any other instrument in which the microscope travels could have been used provided the focusing motion allowed sufficient room for the vertical illuminator.

For this measurement it is preferable to move the microscope rather than the stage, as care must be taken to prevent the lenses rocking on each other.

Measurement of Focal Length.—The methods described depend on the use of a measuring microscope to determine the equivalent focal length and back focus of the o.g. field and eye lenses and the equivalent focal length of the complete eye-piece.

As a general rule it is desirable to measure the focal length as the lens will be used. Thus, for an o.g. it is best to measure the back focus and to determine the position of the principal or nodal points, as in this way aberrational errors are reduced, while in the case of an eye-piece covering a considerable field it is preferable to measure the image which the system gives of a large distant object or its equivalent.

Measurement of Focal Length of an o.g.—A distant object (or collimator) is used and a plane mirror employed to direct the beam vertically upwards; a fixed aperture is placed on the stage of a measuring microscope and the lens to be tested rested on this aperture.

The microscope is set vertically, and is provided with a long focusing scale of, say, 150

mm. The microscope is focused, first, on the image of the distant object; secondly, on the back surface of the o.g., and, thirdly, on the aperture below the o.g., the horizontal motion being used to bring the two opposite edges of the aperture on the cross lines in turn.

The readings on the vertical scale being V_1 , V_2 , V_3 , and on the horizontal scale H_1 and H_2 .

We have back focus = $V_1 - V_2$.

$$\text{Equivalent focal length} = V_1 - V_2 \frac{a}{H_1 - H_2}$$

where a is the diameter of the aperture.

It is immaterial whether the aperture be in contact with the o.g. or not provided the separation be not large.

Field Lens.—A similar method may be applied to the field lens, except that in this case the curved side is placed downwards and the third reading is made on the lower surface of the lens.

In this case the equivalent focal length = $V_1 - V_2$, and the back focus = $V_1 - V_2$.

Eye Lens.—Procedure as for o.g., the side which is next to the eye being placed downwards.

Complete Eye-Piece.—A collimator whose o.g. gives good definition over a large field is used, an object having a length, half, or a third the focal length of the collimator o.g. is placed at the focus.

The e.p. to be tested is placed so that the stop of the collimator o.g. is at the place where the Ramsden circle of the eye-piece would be when in use, i.e., at the eye point of the eye-piece.

The e.p. should then be as free from distortion as it is in actual use.

The size of the image formed by the eye-piece is measured by a measuring microscope, and is multiplied by two or three to give the equivalent focal length of the eye-piece according as the object is half or third of the focal length of the o.g.

(When no other collimator is available a suitable collimator can be made by removing the eye-piece of one barrel of a prism binocular and placing at the position usually occupied by a graticule an object of diameter one-tenth of the focal length, replacing the eye-piece, carefully focusing up this object and focusing the binocular on a distant object. When a light is placed beyond the eye-piece a suitable collimator is obtained, but in this case the image size should be multiplied by ten.)

The adjustments are simplified if the collimator can be arranged vertically and the e.p.



FIG. 7.

simply placed on a diaphragm over the collimator o.g.

The whole of the focal length measurements that may be required can be readily made on the one instrument, which may be the one used for curvature measurements, provided the range of focus in the axis direction be sufficient.

Measurement of Angles of Prisms.—The method used for finished prisms is that originally described with Mr. H. S. Ryland to the Optical Society*.

The apparatus consists of an auto-collimating telescope with a plane surface normal to the

* Trans. Optical Society, Vol. VI., 1904-5. p. 34.

axis against which the prism can be held. Fig. 7 shows the arrangement for testing the right angle. Light diverging from the brightly illuminated slit falls on the prism, and is reflected back as two beams slightly inclined to each other. The angle between the two reflected beams is observed by means of a scale in the e.p. and the value of the angle so obtained divided by $4n$ —(n is the refractive index of the prism)—gives the error in the right angle. Whether the angle is greater or less than a right angle can be determined by drawing out the eye-piece; if the two images come together as the eye-piece is drawn out, the angle of the prism is greater than a right angle.

Instead of moving the eye-piece we may pass a piece of coloured glass across the beam between the eye-piece and the eye. As the glass gradually cuts off the beam one and then the other image will become coloured; if the first image to be coloured is on the side at which the glass enters the beam the angle will be less than a right angle.

As an illustration of the measurement, an o.g. of 277 mm. focal length with a millimeter scale in the e.p. each millimeter represents an actual angle of 12.4 minutes, i.e., an error in the right angle of 2 minutes. ($n = 1.55$.)

Observation, *test of right angle* 4 mm. separation; image cut off on the opposite side to the beam, leaving the eye-piece—i.e., angle—greater than 90° . Angle of prism = $90^\circ + 8'$.

At the same time it was noticed that the image reflected from the front face of the prism was deflected upwards 3 mms. compared with the two bright images. This indicated a pyramidal error—i.e., the edge was not parallel to the front face, the vertex was upwards, and the effective

37.2

angle was — minutes—i.e., 12 minutes.

$2n$

The same apparatus was used to test the equality of the two 45° angles; one right-angle

face was placed normally to the axis of the telescope, two images were observed separated by 3 mms.

The angle between the reflected beams divided by $4n$ gives the amount by which each differs from the mean.

Since $A = 90^\circ 8'$

$$B + C = 89^\circ 52'$$

and the mean is $44^\circ 56'$;

now B is $6'$ greater and C is $6'$ less than the mean, so that

$$B = 45^\circ 2', C = 44^\circ 50', \text{ and } A = 90^\circ 8'.$$

In this way the three angles may be tested.

If the o.g. be large enough to cover the whole prism the definition given by the prism can be readily tested by observing the brightly reflected images when the right angle is being tested; any astigmatism of the image would indicate that the reflecting faces were curved, or that the glass was badly strained. The prism is being used substantially, as it would be in a prism binocular. If, however, the prism is to be used as in a periscope, special attention should be paid to the definition while testing for equality of angles.

If, however, one is only concerned with the testing of angles, a shorter focal length o.g. would do, say 111 mm.; so that, nearly enough for a limit test, a 114 to 116 mm. prism binocular o.g. might be used. In this case 1 mm. would represent 5 minutes error in the angle A .

It is not even necessary to have a regular telescope, as an o.g. of this character may be mounted on a board which carries a plane plate for the prism under test and a 90° prism having one of the right-angle faces blacked off except for a bright cross of, say, 1 mm. width.

This prism is placed slightly out of square, so that a source of light at the side may be used to illuminate the slit (fig. 8).

Above the prism is a small piece of fine

ground glass carrying a scale showing half-millimeters.

If the apparatus be adjusted so that the illuminating prism and the ground glass are at the focus of the o.g. two images will appear on the ground glass, and by their separation the error in the right angle can be determined. If it be desired to test the pyramidal error the prism must be rotated till the reflection from the front face falls on the ground glass also. The difference in the two images measured vertically indicates the error.

In this case it is necessary to screen the ground glass from stray light, as the front surface image is not very bright.



FIG. 8. A—Plan. B—Elevation.

The method of testing the equality of the two angles is applicable to any isosceles prism, and, in fact, to all cases where the light passing through a prism is incident normally on both front and back surfaces—such as "roof" prisms and other special reversing prisms of this character.

In the case of roof prisms there will be a doubling of all reflected images, which will indicate the error of the roof angle.

The same general principles may be applied to tests made in the course of manufacture.

Errors in the prism while smooth or rough may be tested by springing two plane mirrors on to the faces containing the right angle so

that the reflecting surfaces extend past the end of the prism.

The right angle between these faces can then be tested exactly as if they were faces of a polished prism, except that the angle between the two reflected beams is 4 times the error of the prism instead of $4n$ times the error.

To test the equality of the angles it is necessary to spring a plane unsilvered plate against one of the right-angle faces and mirrors against the other two faces. Once again the angle between the two reflected beams gives 4 times the

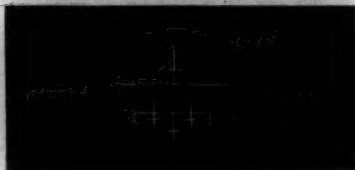


FIG. 9. View of Slit Plane.

amount by which each of the angles departs from the mean.

To test the pyramidal error we may measure the angle between each face and the end, or we may use a plane plate on the hypotenuse face and mirrors on the other two faces.

In conclusion, the author desires to express his indebtedness to Mr. C. L. Redding for his help in testing some of the methods described, and in arranging demonstrations of these methods. His thanks are also due to Mr. W. H. Fincham for his assistance in connection with the demonstrations.

Discussion.

DR. CLAY said he had been very much interested in Mr. Chalmers' very lucid explanation of the methods he had devised for these practical tests. With regard to Mr. Chalmers'

remark that he had found that the limit of accuracy in the method he had described for measuring the curvatures of surface was settled by the difficulty of reading the scale, Dr. Clay said that as the ordinary micrometer could be read without much difficulty to one or two 10,000ths of an inch, it occurred to him that by using the moving part of that, which, as a matter of fact, could be bought as a separate instrument, and holding the lens support against it by a spring, one ought to be able to get any degree of accuracy required. A ball end might be added to Mr. Chalmers' sliding stage, so as to make quite sure of getting proper point contact which did not vary while the micrometer was being rotated. Mr. Chalmers' method was something like one he had seen in Berlin for measuring the focal length of a lens, in which the lens was moved sideways and the the images of suitable objects below the stage were brought successively into line with the axis. One other thing he might perhaps mention, and that was in reference to the measurement of the curvature of a surface which was very nearly plane. He was sure Mr. Chalmers would not contradict him when he said that the adjustment of these instruments so as to get the Newton rings satisfactorily was not a thing that the ordinary workman would find very easy. It was a thing which one gave advanced students to do, and at the first trial they sometimes spent a whole afternoon before succeeding. (Mr. Chalmers said that in the case of the instrument on the table the adjustment was very easily made.) He had had occasion some years ago to measure the curvature of a toric surface which was very nearly plane, and which, as it happened, was silvered. He was desirous of not spoiling the silvering. He had used simply an ordinary telescope of about 2" aperture, which he focused upon an astigmatic chart at the far end of the room, and then turning the axis of the telescope through a right angle, he

put this nearly plane surface which he wanted to measure, at an angle of 45° , so as to reflect the image of the chart again into the telescope. Of course, if the surface had been really plane, and he had kept his total distance the same—that was to say, if the original distance from the chart to the objective was exactly the same as the new distance from the chart to the mirror and on to the objective, he ought not to have had to alter the focus of the telescope. But, owing to the curvature of the surface, he found that he had to adjust it, and by turning the mirror round in its own plane until the axis of the cylinder was first of all horizontal and then vertical, he found that the two corresponding readings of the telescope, one for each position, which were due to the focal lines obtained by reflection at an angle of 45° . Then, knowing the focal length of the objective and the amount of the change of focus of the telescope, it was quite easy to calculate the two radii of curvature of the surface. He thought the surface he was measuring had a curvature on one meridian of something like 20 meters, and 12 meters on the other, so that it was very nearly a flat surface; the movement of the eyepiece of the telescope was about $\frac{1}{2}$ ", so that it was easy to obtain a fairly high order of accuracy in the final results. It was five or six years ago, and possibly his memory of the details of his observations was not exactly right; but the experiment was one that did not require any high degree of skill, and for which almost anyone would have the apparatus.

MR. CONRADT said he had much enjoyed hearing Mr. Chalmers' paper, and should certainly profit by some of it. He did not know whether he would be in order making a very slight addition to it, but there was one point Mr. Chalmers had not mentioned at all, namely, purely naked-eye methods, which, of course, were not, strictly speaking, measuring methods. But he had lately come across an extremely

interesting method of that kind, and it had happened, as such things usually did, by accident. In the process of designing he had come across the well known "optical square" reflection in the right-angled prism as a ghost which had to be laid. He thus became interested in this particular double reflection and eventually found it to supply an extraordinarily searching test of the angles of the ordinary right-angled prism.

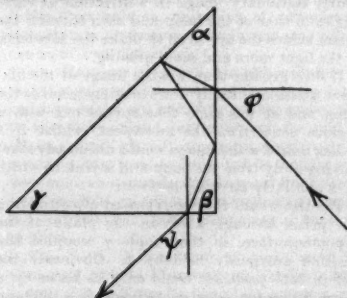


FIG. 1.

Referring to a diagram (fig. 1), he pointed out that the angle between the incident ray and that emerging after the two reflections was constant only on condition that β be equal to twice α . If this condition is not fulfilled rigorously the angle between the entering and leaving ray becomes variable; the variation being barely noticeable as long as ϕ and ψ are well below a right angle, but becoming larger and larger as these angles approach 90° .

Analytical treatment yielded the simple approximate formula

$$\psi - \phi = (\beta - 2\alpha) \frac{57.3 \sqrt{n^2 - 1}}{90^\circ - \phi}$$

in which n stood for the refractive index of the glass. It was obvious that the error of the prism ($\beta - 2\alpha$) became multiplied by a very large factor as $90 - \phi$ approached zero (or grazing incidence), hence the extraordinary sensitiveness of the test.

Owing to the faintness of the optical square reflection in an unsilvered prism, a lamp, best of all a metal filament lamp, formed the best object, the test being carried out by finding the nearly stationary image in a direction at right angles to that of the lamp and then turning the prism before the eye so as to make the incidence of the light more and more grazing.

If β be greater than 2α the image of the filament would eventually start moving towards the lamp and at the same time spread out with a curious concertina-like movement, whilst if β be less than 2α the image would ultimately start moving away from the lamp and shrink in width in a similarly striking manner.

The test would then be repeated after turning the prism through 180° in the plane of the hypotenuse-face, so that angle γ occupied the position previously held by α . Obviously the test in *both* positions could only be borne by a prism having $\alpha = \gamma = 45^\circ$ and $\beta = 90^\circ$, in other words, by a perfect one; so far he had not found one yet.

Prismatic Telescope Prisms.

MR. EVERITT said there was one very simple method which Mr. Chalmers had not mentioned, and which was possibly of interest in connection with the method Mr. Conrady had just described; and that was in reference to the difference between the angles. The angles should be 45° and they should be equal. If one looked at an ordinary prism binocular through an ordinary high power telescope one got a certain amount of dispersion. If there was an error one knew the prism is too large; if not, the prism was good enough for most pur-

poses. That was a method which seemed particularly applicable to the degree of accuracy that was wanted for a prism binocular, and a telescope was a thing everyone possessed.

MR. CHALMERS pointed out that he used practically the same method in deciding the definition of the prism with an ordinary collimating telescope, except that he was testing the prism in just the way it would actually be used.

MR. EVERITT: Yes, but we did not hear anything about the dispersion due to the prism.

MR. CHALMERS was obliged to Mr. Everitt for mentioning dispersion, which was one of the causes of bad definition.

MR. EMERSON expressed his pleasure that the question of workshop methods in making lenses had been taken up, and he hoped that from time to time they might hear more of the subject. (Hear, hear.) Mr. Chalmers' methods were all for measuring focal lengths, but he thought some very accurate methods of measuring focal powers were possible, and that some method of measuring differences of focal power might be easily applied.

MR. F. G. MARSH said he had found Mr. Chalmers' method of testing by Newton's interference bands for spherical plate curves a very fine method of making a rough shot at a proof plate of any diameter or any radius required. He had in mind a case of a very large radius 5" plate. He had no idea of the curvature of the tool, with nothing to guide him, so he made by Mr. Chalmers' "sag" method an approximate plate which he had had tested by an optical method in the laboratory, and that was returned to him with a definite amount of error. From that he had worked backwards by the aid of Newton's rings, and within two tries he had obtained the result. A very useful method of testing a prism when it was in a block with the three sides polished and of getting a rough idea of how the work was getting along, was by putting the light about 12 inches away from the

prism faces, and looking direct into the prism faces—that was provided they were taking the short sides last. One saw the image of one's eye reflected, and could test the prisms finally and definitely whether they were going to pass or not from that point. If there was a pyramid effect in the prism or the two surfaces were not 90° to a common base, the reflection from the pupil of the eye was shown as two bright specks one upon the other. If the 45° were not correct the reflection was shown to the right or left, according to how one was looking at the prism, and if both were in error it would come to the right or left corner or the lower corner and it could be seen just where the angles wanted correcting from that point by simply looking at the reflection from the eye.

THE PRESIDENT hoped that that night's demonstration was simply the first of a series. (Hear hear.) The subject was an exceedingly important one, and he thought one of the first objects of the Society at the present time should be to sort out and sift these various methods for the benefit of the trade generally. (Hear, hear.) There was a good old fable which it would be as well to bear in mind when considering this question. A fox had a long conversation with a cat, in the course of which he boasted of the thousand and one tricks he was able to resort to in order to evade the hounds. The cat listened very meekly, and was almost ashamed to say that it had only one. Immediately after, the hounds appeared in sight. The fox proceeded to put his thousand and one methods of escape into practice, and eventually lost his brush. The cat, on the other hand, proceeded to put its one way into practice, namely, that of climbing a tree, and escaped. (Laughter.) Now in such matters as the determination of focal lengths, for example, it was important to have one good workshop method, rather than to have a dozen methods which were not so good; it was important to have one good,

simple, effective method which one could depend upon, and to stick to it. And he hoped that before the meetings for the present year were concluded, they would have been able to select from a considerable number of methods which, he hoped, would be submitted to them from time to time, those methods which were really simple and were best adapted for the requirements of a small, modern workshop. They were not there, of course, to cater for large and ambitious optical instrument makers; it was the small maker who was more particularly in want of the assistance they were anxious to give; and for that reason he had been especially pleased with the naked-eye method of Mr. Conrady for examining prisms. Such methods were exceedingly valuable, because they enabled one to pick up a prism from the bench, look at it, and form a good idea of its optical value merely from a naked eye inspection. That reminded him of an application of what a friend of his had already christened the "evil eye." If one took up a rectangular prism and looked normally into the hypotenusal face, one ought to see one eye only. If, however, the prism is not angle-true, then the eye seen is either too long or too short in its horizontal dimensions. If now the prism be gradually removed to a greater distance from the eye, then the image of the latter will in the former case split up into two eyes and in the latter case disappears altogether. He hoped that these so-called "naked eye" methods would form an important feature of the final collection which the Society would be able to get together for the benefit of optical munition makers. In dealing with the question of focal lengths for binoculars, Mr. Chalmers had called attention to a point which was certainly very important. There was no doubt that when two objectives had to be paired, as in the case of a binocular instrument, it was important that not only the back focal lengths should be equal but the focal lengths themselves.

If the back focal lengths were equal, of course, they fitted mechanically into the instrument itself, but if the focal lengths were different, a differently magnified picture was presented to one eye from that which was presented to the other; but if the focal lengths were equal and the back focal lengths unequal, then when one picture was in focus the other would be out of focus.

As regards the necessity for the work which the Society proposed to do there could not be, he thought, two opinions. Not long ago an optician of his acquaintance had sent to him a set of negative eyepieces for an astronomical telescope with the request that he would determine their focal lengths. The optician knew no better way of getting these focal lengths than finding the focal length of each lens separately and then applying the equation for a separated pair of lenses. Unfortunately, however, the crudity of the results obtained in this way was further enhanced by the displacement of the decimal point. The focal lengths, therefore, which should have been engraved in millimeters were engraved in centimeters, and, therefore, made ten times too great. Needless to say, the eyepieces were returned with the satirical remark that there was "obviously something wrong." He had no doubt whatever that during the next week or so many of them would be carrying lenses and prisms about in their pockets, and would investigate for themselves the various naked-eye methods which had been suggested that night; and possibly in the process of doing so they might discover other methods. But let him give them one word of warning.

It was not always safe in these days to go into a railway carriage and to get into a corner and become engrossed in a bit of glass, and squint through it in all directions with a distraught air. Some years ago a student of Birkbeck College had rendered himself the sub-

ject of a good deal of suspicion in that way. A gentleman had come to the College, and said: "You have a very rum chap here; I happen to come down in the train with him nearly every morning, and he simply dives into the carriage, goes into the opposite corner, and sits down gazing into space in a most extraordinary way. And his madness has taken a most severe turn during the last few days, because now, when he gets into the corner of the carriage, he makes the most extraordinary motions with his hands. What is the matter with him?" Well, the explanation was perfectly clear and simple. Dr. Clay, who was present with them that night, was at that time lecturing upon the form of the wave-surface in biaxial crystals, and the student was merely trying to figure out what the intersections of that surface were with the three principal planes. (Laughter.)

Reply.

MR. CHALMERS, replying on the discussion, said there was one point he wished to call attention to which did not arise out of the discussion, namely, that although he had a great deal of apparatus there that night, it was practically all the same apparatus repeated; and all the measurements he had illustrated, with the exception of those on prisms, could be done with one piece of apparatus. There was a naked eye method of checking the equality of the angles of the prisms, which consisted in taking two reflected images, one reflected from the first surface and one from the second surface. The method he had shown on the instrument was really suitable for a naked eye method of checking the equality of the two 45° angles, and it was better to have a certain amount of obliquity to the light coming on rather than to view with the images of the eye itself in that particular test. He had been very much interested in Mr. Conrady's method and thought it should be possible to make it a measuring method with-

out any great complication. With regard to the other points raised, he would only say that he hoped many of his audience would avail themselves of the opportunity of inspecting the apparatus at close quarters, and he would then welcome any further criticisms. There was only one point with regard to which he would utter a warning, and that was that one particular tool had been badly scratched, and, therefore, the actual measurements taken with it would probably not be so accurate as would otherwise have been the case.

In reply to Dr. Clay's remarks *re* Newton's rings, the use of a vertical illuminator, the use of central contacts only, and the restriction of the method in the case of contacts such as those described, made the adjustment quite rapid, the only special precaution needed was to place the instrument on a steady stand.

APPARATUS USED FOR THE TEACHING
OF OPTICS AT THE CAVENDISH
LABORATORY, CAMBRIDGE.

By DR. G. F. C. SEARLE, F.R.S.

Read 10th May, 1916.

DR. SEARLE exhibited and described the apparatus used for the following experiments in his class at the Cavendish Laboratory, Cambridge. The methods are briefly described in the following abstracts; the references indicate where fuller information may be found. His object was not to exhibit elaborate apparatus but to show how quite simple apparatus might be used for purposes of instruction.

§ 1.—MEASUREMENT OF ANGLE AND REFRACTIVE
INDEX OF A PRISM OF SMALL ANGLE.

Method 1.—The goniometer used in the experiments is shown in fig. 1; it was designed

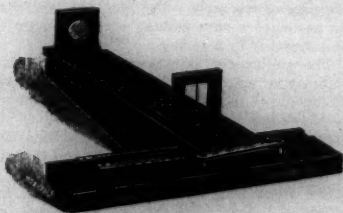


FIG. 1.

in conjunction with W. G. Pye and Co. The base is furnished at one end with a spherical pivot, and at the other with a cross-bar carry-

ing a scale. Angles are measured by means of an arm which turns at one end about the pivot, while the other end moves over the scale on the cross-bar. The optical system consists of a lens, about 35 cm. in focal length, fixed to the arm above the pivot and of a vertical wire held in a frame at the other end of the arm; the frame is adjusted so that the wire is in the focal plane of the lens.

The pivot is a phosphor-bronze ball attached to an adjustable fitting. The ball enters a conical hole turned out of a block attached to the arm; the other end of the arm carries two feet which rest upon the cross-bar.

The scale is divided into millimeters and the ball is adjusted so that its centre is 40 cm. from

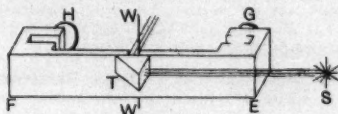


FIG. 2.

the edge of the scale. The readings are taken by means of a wire passing across an opening and stretched by a spring.

The goniometer is converted into an auto-collimating instrument by the addition of the fitting shown in fig. 2. The bar EF is clamped to the frame holding the vertical wire of the goniometer by the screws G, H. A vertical slit is cut in the upper edge of the bar and this slit is covered by a small totally reflecting prism T. When light from a source S falls upon the prism, it is internally reflected and passes out through the slit past the vertical wire WW'. The beam of light then passes through the lens of the goniometer; if it suffers reflexion at a suitably placed plane mirror, those rays which fall normally upon the mirror will retrace their

paths, and, after passing a second time through the lens, will come to a focus at the point in the focal plane from which they started. Other points in the focal plane will have *inverted* images in that plane and thus an inverted image of the slit and wire will be seen. By adjusting the arm of the goniometer or by turning the mirror about a vertical axis, the image of the wire may be made to coincide with the wire itself.

The goniometer does not measure angles directly but their tangents. If the indicating wire crosses the edge of the scale at a distance x cm. from the central division, and if the angle between the displaced and the central positions of the arm is θ radians, then $\tan \theta = x/40$, if the distance from the pivot to the edge of the scale is 40 cm. When $\tan \theta$ is known, θ can be found in degrees by trigonometrical tables or in radians by the series

$$\theta = \tan \theta - \frac{1}{3} \tan^3 \theta + \frac{1}{5} \tan^5 \theta - \dots (1)$$

The following table enables θ to be found when $\tan \theta$ is known. It gives c , the quantity which must be subtracted from $\tan \theta$ to obtain the value of θ in radians.

$\tan \theta$	c	$\tan \theta$	c	$\tan \theta$	c
0.01	0.00000	0.10	0.00033	0.19	0.00224
0.02	0.00000	0.11	0.00044	0.20	0.00260
0.03	0.00001	0.12	0.00057	0.21	0.00301
0.04	0.00002	0.13	0.00072	0.22	0.00345
0.05	0.00004	0.14	0.00090	0.23	0.00393
0.06	0.00007	0.15	0.00111	0.24	0.00446
0.07	0.00011	0.16	0.00134	0.25	0.00503
0.08	0.00017	0.17	0.00161		
0.09	0.00024	0.18	0.00191		

Let ABC (fig. 3) be a section of the prism by a principal plane and let the angle BAC of the prism be i radians. Let P be a point on the face AC and let PK be perpendicular to the face AB. Let the ray KP on passing out of the glass into the air be refracted along PQ, making an angle θ with the normal PN. Then, if μ be the refractive index of the prism,

$$\sin \theta = \mu \sin i \dots \dots \dots (2)$$

The directions PN and PQ are identified by optical means, for PN is the direction of a parallel beam which returns along its path after reflexion at AC, and PQ is the direction of a

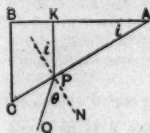


FIG. 3.

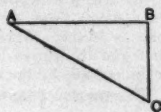


FIG. 4.

beam which returns along its path after two refractions at AC and one reflexion at AB.

The angle i of the prism is easily found. For, if the prism be turned through two right angles about an axis perpendicular to the plane of the face AB, so that the section of the prism comes into the position shown in fig. 4, the normal to the face AC will then make an angle $2i$ with its former direction. Hence this motion of the prism will entail a change of direction of $2i$ in the parallel beam of rays which falls normally upon the face AC.

The prism is held in a simple geometrical clamp (fig. 5). Three bicycle balls are soldered to a brass plate and one face of the prism (AB in fig. 3) is kept in contact with the spheres by two steel springs. This spring clamp is attached to a horizontal rod carried by a heavy stand.

The screw at the centre of the brass plate allows the plate to be adjusted so that the ledge at the bottom of the plate is horizontal. If the ends of the prism have been made perpendicular to the refracting edge (which is not always the case), this will secure that the prism is properly placed.

The goniometer is fixed so that light from a sodium flame passes through its reflecting prism. The clamp holding the prism to be tested is placed close to the lens of the goniometer and is turned about the horizontal rod so that, when the arm of the goniometer is suitably directed, an image of the slit is seen just above the totally reflecting prism. There are two positions of the

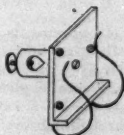


FIG. 5.

arm in which an image is seen. In one position the light is reflected at the front face of the prism, in the second it is reflected at the back face; the first position corresponds to PN in fig. 3 and the second to PQ.

The marked face of the prism is placed against the spheres and the two goniometer readings corresponding to reflexion at the front face and to reflexion at the back face are taken. The angle between the two positions of the arm is the angle θ in fig. 3. Keeping the marked face still in contact with the spheres, the prism is turned through 180° about a horizontal axis perpendicular to the plane of the spheres and the observations are repeated, thus furnishing a second value of θ ; care must, of course, be taken not to move the clamp. The angle between

the two positions of the arm corresponding to reflexion at the front face is equal to $2i$. The refractive index is then found by the formula

$$\mu = \sin \theta / \sin i \dots\dots\dots (3)$$

An Alternative Process.

Method 2.—In the above no assumption was made as to the magnitude of the angle of the prism. In the following experiment, however, it is necessary that the angles concerned should be "small."

When a ray passes with small angles of incidence and refraction through a prism of small angle i radians and of index μ , the deviation, D radians, is given by the formula

$$D = (\mu - 1) i \dots\dots\dots (4)$$

The prism is placed with its edge vertical in a tank with parallel glass sides containing water of refractive index μ_1 . If the sides of the tank are plates of plane-parallel glass, they will not affect the deviation. If a ray passes nearly symmetrically through the system, and if D_1 be the resultant deviation, and if D_1 be counted positive when it is in the same direction as that due to the glass prism alone, we have

$$D_1 = (\mu - \mu_1) i \dots\dots\dots (5)$$

Equations (4) and (5) enable us to find i and μ , if we know μ_1 and observe D and D_1 . Since by (4) and (5)

$$(\mu - 1)/(\mu - \mu_1) = D/D_1 \dots\dots\dots (6)$$

$$\text{we find } \mu = (\mu_1 D - D_1)/(D - D_1) \dots\dots\dots (7)$$

Subtracting (5) from (4), we find $D - D_1 = (\mu_1 - 1) i$, and thus

$$i = (D - D_1)/(\mu_1 - 1) \dots\dots\dots (8)$$

The measurements are made by the goniometer. The collimator AB (fig. 6) has cross wires at A in the focal plane of the lens B (of 15 to 20 cm. focus). The tank C has sides of plane glass, but the two sides need not be

accurately parallel. The collimator is illuminated by a sodium flame F . The tank contains water of refractive index μ_1 .

The prism is first placed between the collimator and the goniometer, the tank being removed, and a reading of the goniometer is taken. The prism is then turned through 180° about a vertical axis and a second reading is taken. The difference between the two readings corresponds to $2D$.

The tank is then put into place and is filled with water, and the prism is placed in the tank, as indicated in fig. 6. The goniometer reading is taken and then, without disturbing the tank, the prism is turned through 180° and a second reading is taken, the difference corresponding to $2D_1$.

If α and α_1 be the changes of goniometer reading corresponding to $2D$ and to $2D_1$, the angles

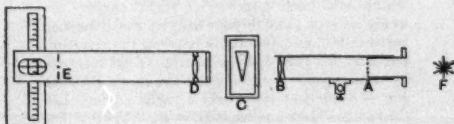


Fig. 6.

will be so small with a suitable prism that we may write

$$D = \alpha/2l, \quad D_1 = \alpha_1/2l,$$

where l is the length from the centre of the pivot to the edge of the goniometer scale, and the angles are expressed in radians. Equations (7) and (8) then give μ and i . For water

$$\mu_1 = 1.3334 - 0.00009(t - 15),$$

where t is the centigrade temperature. For temperatures near 15°C . it will suffice to take $\mu_1 = \frac{4}{3}$ and $\mu - 1 = \frac{1}{3}$.

If the auto-collimating device of the goniometer

meter is used, the collimator may be dispensed with. A plane mirror is placed with its face approximately perpendicular to the axis of the goniometer arm when the latter is in its central position. The goniometer wire is then made to coincide with its own image in each case.

The two methods are convenient for testing prisms of small angle such as those used in spectacles.

§ 2.—DETERMINATION OF THE ANGLE BETWEEN TWO NEARLY PERPENDICULAR MIRRORS.

If two plane mirrors are inclined to each other at exactly a right angle, any ray which suffers two reflexions, one at each mirror, leaves the second mirror parallel to its original direction. When the angle between the mirrors is not exactly 90° , the initial and final rays are no longer parallel. If the initial ray is parallel to a principal plane of the mirrors, i.e. to a plane which cuts both mirrors at right angles, the angle between the two rays is twice the difference between 90° and the angle between the mirrors. Let AB, AC (fig. 7) be a section of the mirrors by a principal plane and let the angle BAC be $\frac{1}{2}\pi + \theta$, so that it exceeds a right angle. Let the ray be PQRS and let $BQP = \alpha$. Then, if PQ and SR meet in F, $FQR = 2\alpha$. Further,

$$ARQ = \pi - AQR - QAR = \pi - \alpha - (\frac{1}{2}\pi + \theta) = \frac{1}{2}\pi - \alpha - \theta,$$

$$\text{and hence} \quad FRQ = 2ARQ = \pi - 2\alpha - 2\theta.$$

$$\text{Thus } PFS = \pi - FQR - FRQ = 2\theta.$$

In fig. 7 the ray PQ falls first on the mirror AB. If, however, a ray P'Q' parallel to PQ falls first on AC, it will give rise to a ray R'S' which will be inclined to the ray P'Q' at the angle 2θ , but the deviation of R'S' from P'Q' will be in the opposite direction to the deviation of RS from PQ. Hence a beam of rays parallel to PQ, which is wide enough to fall upon both mirrors, gives rise to two beams parallel to RS and R'S', these beams deviating by equal angles

but in opposite directions from PQ. In this case we could find by how much the angle BAC differs from 90° , but we could not decide whether BAC is greater or less than 90° . To do this, we must ensure that the incident light falls on only *one* of the two mirrors, say on AB.

Thus we can find the value of θ if we can measure the angle PFS between the initial and final rays. This can be done by the auto-collimating goniometer.

The apparatus is arranged as in fig. 8. Here AB, AC are the two mirrors, LM is the lens of the goniometer, T is the totally reflecting prism of the auto-collimating device (see fig. 2), and

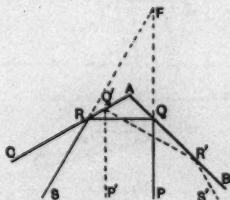


FIG. 7.

S is a small gas burner. When the burner is so close to T that the rays which pass through T fill the lens LM with light, two images of the wire W will be seen, the wire being midway between the images. If, however, the burner is placed at some distance from T in such a position that the rays transmitted by T fall only on the mirror AB (as indicated in fig. 8), only one image will be seen. If, as in fig. 8, this single image U is to the left of W, then the angle BAC is greater than a right angle; if U is to the right of W, the angle is less than a right angle. I owe this suggestion to Mr. S. D. Chalmers. When the burner is nearly in the

proper position, the adjustment may be completed by moving the goniometer arm.

For measuring the angle, a glass millimeter scale is held against the frame supporting the vertical wire W of the goniometer, so that W is in contact with the divided face of the scale. To increase the distance to be measured, the flame should be placed so that *both* images are visible. It will be found that the two images move together with W when the goniometer arm is moved.

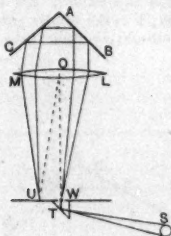


FIG. 8.

If O (fig. 8) be that nodal point of the lens which corresponds to the focal plane WU, then $OW = f$, the focal length. By the property of nodal points, OW and OU are parallel to the two parallel beams on the other side of the lens. Thus OW and OU correspond to PQ and RS in fig. 7, and hence in the case of fig. 8, the angle (ϕ) between the mirrors is: $\phi = \frac{1}{2}\pi + \frac{1}{2}\angle UOW$. If $WU = y$, then

$$\phi = \frac{1}{2}\pi + y/2f. \dots\dots\dots (20)$$

§ 3.—DETERMINATION OF THE ANGLE OF A PRISM OF NEARLY 90° .

The method of § 2 is easily adapted to the measurement of such a prism, provided the face opposite the reputed right angle is polished, as

it must be if the prism is intended to change the direction of rays by refraction. Let ABC (fig. 9) be a principal section of the prism, the angle A being nearly $\frac{1}{2}\pi$. Let PQRSXY be the path of a ray, the incidence at Q being nearly normal. By § 2, if $BAC = \frac{1}{2}\pi + \theta$, the angle between QR and XS is 2θ , and, if θ is positive, the point of intersection of QR and XS is on the same side of RS as A is, and this is true whatever the angle of incidence of QR at R. If BAC is exactly $\frac{1}{2}\pi$, QR and SX are parallel and then PQ and XY are parallel also. When, however, QR and SX are not parallel, we must take account of the refractions at Q and X. Let MQM', NXN' be the normals at Q and X. Then

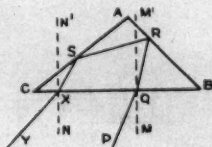


FIG. 9.

$\sin PQM = \mu \sin RQM'$ and $\sin YXN = \mu \sin SXN'$, where μ is the refractive index of the prism. Now it is easy to place the prism so that the ray PQ which falls upon it from the auto-collimating goniometer is nearly normal to BC, and then, if BAC is nearly $\frac{1}{2}\pi$, the ray XY will also be nearly normal to BC. We may then use the angles instead of their sines and then

$$PQM = \mu RQM', \text{ and } YXN = \mu SXN',$$

and thus the angle between PQ and YX is μ times the angle between QR and SX, i.e., it is $2\mu\theta$. The angle between PQ and YX is measured by the goniometer by the method of § 2 in exactly the same way as for the double mirror. If this angle be ψ radians, then $\psi = 2\mu\theta$, and the error in the angle BAC is

$\psi/2\mu$ radians. The method indicated in fig. 8 is used to decide whether BAC is greater or less than a right angle. The refractive index of the prism must be found by an ordinary spectrometer.

(See "Experiments with a Prism of Small Angle." Proceedings of the Cambridge Philosophical Society, Vol. XVIII., p. 155.)

§ 4.—MEASUREMENT OF FOCAL LENGTH BY GONIOMETER.

The goniometer (§ 1) may be used to measure the focal length of any coaxial system of lenses by a method depending upon the properties of the nodal points.

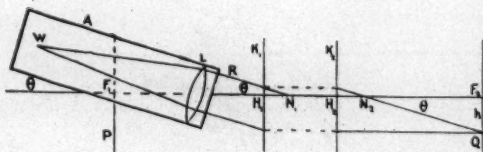


FIG. 10.

In the diagram (fig. 10) let F_1P and F_2Q be the two focal planes of the system, H_1K_1 and H_2K_2 the two principal or unit planes, and N_1 and N_2 the two nodal points. The lenses forming the optical system under test are *not* shown in the diagram. Let $F_1H_1 = f_1$, and $F_2H_2 = f_2$, then f_1 and f_2 are the two focal lengths of the system. The nodal points are defined by the property that any incident ray QN_2 , whose direction passes through N_2 , gives rise to an emergent ray N_1R whose direction passes through N_1 and is parallel to the ray QN_2 . It can be shown that $N_2F_2 = f_1$ and that $N_1F_1 = f_2$. When the medium is the same at both ends of the system, the nodal points N_1, N_2

coincide with the principal points H_1 , H_2 , and then $f_1 = f_2 = f$.

The point Q in the focal plane through F_2 will emit a pencil of rays which will give rise to an emergent beam of parallel rays, as indicated in the diagram. If the goniometer arm A be properly adjusted, this parallel beam is brought to a focus on the wire W by the lens L . Similarly, a point at F_2 gives rise to a parallel beam which may be brought to a focus at W by turning the arm through θ radians, the angle between the two beams. But, by the property of the nodal points, the angle QN_2F_2 is also equal to θ . But, if $F_2Q = h$, we have, for small angles, $\theta = h/N_2F_2 = h/f_1$, and

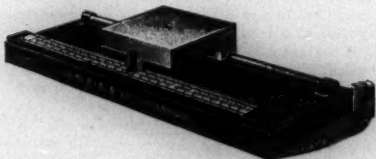


FIG. 11.

thus $f_1 = h/\theta$. Since we know θ , f_1 can be calculated as soon as h is known.

The distance h or F_2Q is measured by a glass scale placed with its divided face in the focal plane; it is in adjustment when one of the dividing lines is sharply focused (without parallax) upon the wire W . The adjustment is easily made if the scale be mounted on a sliding carriage similar to that shown in fig. 11. A scale divided to millimeters may be used for optical systems with focal lengths exceeding 5 cms. Satisfactory results may be obtained for telephoto systems of any focal length.

The goniometer method may be used for finding the focal length of a microscope objective if a micrometer scale, divided to $\frac{1}{10}$ mm. and

provided with an eye-piece, be substituted for the glass scale. When the micrometer is in adjustment, the image of the goniometer wire will be sharply focused (without parallax) on the micrometer scale. Two readings are taken of the position of the image of the goniometer wire on the micrometer scale for two positions of the goniometer, arm, and the focal length is calculated as before.

The base-board of the carriage carries a steel rod and a steel scale 30 cm. in length divided to millimeters, the rod and the scale being parallel to each other. The carriage is of cast-iron and has three projections on its lower side. In two of these a V groove is cut and

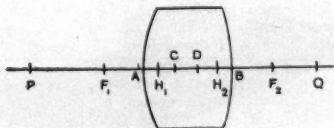


FIG. 12.

these two V's rest upon the steel rod, the remaining projection resting upon the scale.

For many purposes it is convenient to fit a vertical steel rod to the carriage, and to attach objects to the rod by the aid of suitable clamps.

(See "Demonstration of Laboratory Apparatus and Experiments." Proceedings of the Optical Convention, Vol. II., 1912, p. 165.)

§ 5.—MEASUREMENT OF FOCAL LENGTH BY GAUSS'S METHOD.

The method is convenient when the axial length of the lens system does not exceed a few centimeters. The positions of the two principal foci F_1 and F_2 are first found by aid of a distant object or of a plane mirror. Let the axis cut the extreme surfaces of the system in A and B (fig. 12), let F_2 be the focus when

the rays from the distant point fall first on the surface A, and F_1 the focus when they fall first on B. Let $AF_1 = U$, $BF_2 = V$.

If P be *any* point on the axis and Q be its image, then $PF_1 \cdot QF_2 = f^2$, where f is the focal length of the system. Gauss's method, instead of using *any* point, uses the point A as object. If C be the image of A, *i.e.*, the apparent position of A as seen by an observer on the other side of the lens, then $AF_1 \cdot CF_2 = f^2$. If D be the image of B, then $BF_2 \cdot DF_1 = f^2$.

A microscope of low power capable of motion parallel to its own axis is used for the measurements. It may be attached to the sliding carriage described in § 4. A few grains of lycopodium or other powder are placed on the surfaces at A and B, and the axis of the microscope is made to coincide as nearly as possible with the axis of the system. The surface B is turned towards the microscope, which is focused first on the grains at B and then on the images at C (formed by the system) of the grains at A. The displacement of the microscope is equal to the distance between B and C, the image of A. Let $BC = b$ and let b be positive when C and A are on the same side of B.

Since A and C are conjugate points,

$$f^2 = AF_1 \cdot CF_2 = U(V + b).$$

If $AD = a$, we have in like manner

$$f^2 = V(U + a).$$

The positions of the principal or unit points can now be found. For, if H_1 be the principal point corresponding to F_1 , we have $F_1H_1 = f$, and thus

$$AH_1 = f - U, \quad BH_2 = f - V.$$

§ 6.—MEASUREMENT OF FOCAL LENGTH BY MICROMETER METHOD.

A microscope fitted with a micrometer eyepiece may be employed to determine the focal length of an optical system. The method has

been recommended by Mr. T. H. Blakesley (Geometrical Optics, p. 91).

Let F_1, F_2 (fig. 13) be the foci, H_1, H_2 the principal or unit points, and A, B the points in which the axis intersects the extreme surfaces of the system. Let $AF_1 = U, BF_2 = V$. Let Q_1 be a point on the surface near A and let the straight line F_1Q_1 cut the principal plane through H_1 in R_1 . Let $R_1S_1T_1$ parallel to the axis cut the B-surface in S_1 . Since the ray F_1Q_1 passes through the focus F_1 , it gives rise to the emergent ray S_1T_1 , and since the ray F_1Q_1 passes through Q_1 , the image of Q_1 lies somewhere on the straight line $R_1S_1T_1$. Hence

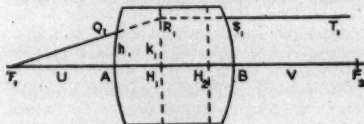


FIG. 13.

R_1H_1 is equal in magnitude to the image of Q_1A .

Let $Q_1A = h_1$ and $R_1H_1 = k_1$. Then, by the similar triangles $R_1H_1F_1$, and Q_1AF_1 ,

$$f_1 = F_1H_1 = U \cdot R_1H_1/Q_1A = U \cdot k_1/h_1$$

Similarly, if Q_2 be a point on the B-surface at a distance h_2 from B , and if k_2 be the magnitude of the image of Q_2B ,

$$f_2 = F_2H_2 = V \cdot k_2/h_2.$$

A strip of tinfoil with clean edges is used to mark the distance Q_1A on the A surface. If the surface be breathed upon, the foil will adhere to it. The eye-piece of the microscope is adjusted so that the micrometer scale is sharply focused. The foil on the A-surface is then viewed *directly* by the microscope, which is adjusted so that the image of the foil is focused,

without parallax, on the micrometer, the micrometer lines being parallel to the edges of the strip. Let the number of scale divisions covered by the image of the strip be m_1 .

The lens system is now turned round and the same strip of foil is viewed by the microscope *through* the system, the number of divisions covered by it being n_1 .

A strip of foil is now placed on the B-surface and the measurements are repeated. When seen directly it covers m_2 divisions and when seen through the lens it covers n_2 .

The distances $AF_1 = U$ and $BF_2 = V$ are measured and also the thickness AB .

The values of f_1 and f_2 are found from

$$f_1 = U.k_1/h_1 = U.n_1/m_1,$$

$$f_2 = V.k_2/h_2 = V.n_2/m_2.$$

The mean of the values of f_1 and f_2 is taken as the focal length of the system, when the medium at the two ends of the system is the same.

§ 7.—THE DETERMINATION OF THE EFFECTIVE APERTURE OF THE STOP OF A PHOTOGRAPHIC LENS.

The stops of a photographic lens are usually marked with the symbols $f/8$, $f/16$, etc. The symbol $f/8$ means that the effective diameter of the stop is one-eighth of the focal length of the lens system. This effective diameter is not the actual diameter of the hole in the diaphragm used to regulate the light entering the camera, but is, in the case of cameras adjusted for landscape photography, the diameter of that incident beam of rays parallel to the axis, which in its passage through the lens system exactly fills the opening in the actual diaphragm.

The “/” notation is not, however, the only system in use. The stops on some cameras are graduated in “U.S.” numbers (Uniform System, not United States). The stops supplied with Kodak cameras are usually marked with the “U.S.” numbers. On this system the stop

denoted by $f/4$ by English makers is taken as the unit, and the numbers are so chosen that the "U.S." number of a stop is proportional to the exposure required. The correspondence between the systems is as follows:—

"f" system	...	$f/4$	$f/8$	$f/16$	$f/32$	$f/64$
Uniform System		1	4	16	64	256

For intermediate stops, the relation may be expressed algebraically. If the "f" number is f/n , we have $f/n = f/(\frac{1}{4}n \times 4)$, and hence the corresponding "U.S." number is $(n/4)^2$. Thus, if f/n corresponds to the "U.S." number 8, we have $(n/4)^2 = 8$, or $n^2 = 128$, or $n = 11.31$; this stop is often marked with the approximate number $f/11$.

When the stop is placed in front of the lens system, as was often the case in the older photographic lenses, its effective diameter for landscape work is simply equal to the actual diameter of the stop itself. If the diameter of the stop is measured and is found to be a cm., and if the focal length of the lens is f cm., the aperture ratio of the stop is a/f and thus, if the "f" number is f/n ,

$$1/n = a/f, \dots\dots\dots(1)$$

and

$$n = f/a. \dots\dots\dots(2)$$

In the case of modern lenses consisting of two separated components, it is advantageous to place the stop *between* the components, as in fig. 14. The effective diameter of the stop will now not be equal to its actual diameter. The effective diameter can be calculated from the actual diameter when the position of the stop relative to the two components and also the optical constants of the components are known, but it would be troublesome to obtain all these data. It is better to treat the lens system as a whole and to arrange the measurements so that there is no need to take the lens system to pieces.

Let S (fig. 14) be the stop and let T_1 be the image of S formed by the front lens L. Then S

is the image of T_1 formed by the same lens. Hence, a ray RQ_1 , which before incidence on the lens L is parallel to the axis AF_2 and is directed to a point Q_1 on the edge of T_1 , will, after traversing L , pass through P , the corresponding point on the edge of the stop itself. Hence it is clear that the distance of the ray RQ_1 from the axis AF_2 is equal to the radius ($\frac{1}{2}\sigma$) of the effective aperture.

If the front lens L is a converging lens, and if the stop S lies between L and the focal plane of L , the image T_1 will be further from L than S is, and consequently the diameter of the aper-

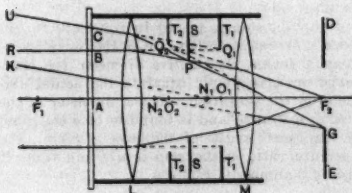


FIG. 14.

ture in the image will be greater than in the actual stop.

In fig. 14 T_2 is the image of the stop S formed by the lens M . Each of the rays RB and UC in its path through the system passes actually, or at least in direction, through (1) Q_1 , (2) P , (3) the edge Q_2 of T_2 . It will be seen that Q_2 is the image of Q_1 by the complete lens system.

Method 1.—The effective diameter of the stop is easily measured by the aid of a microscope mounted on a sliding carriage, such as that shown in fig. 11. The lens system is *firmly* supported with its axis horizontal. The microscope is attached to the carriage so that its axis is at right angles to the direction of motion of the

carriage, and the track on which the carriage slides is placed so that the axis of the microscope is parallel to that of the lens system, the two axes being at the same height above the table. The microscope is then focused through the lens L upon one end of the horizontal diameter of the stop, and the sliding carriage is adjusted so that the image of the edge of the stop is brought to coincidence with the cross-wire of the microscope or with a selected dividing line of the micrometer scale. Some adjustment of the microscope in the direction of its own axis will be required; the adjustment is complete when there is no parallax between the image and the cross-wire. The carriage is then moved along its track so as to bring the image of the other end of the diameter to the cross-wire or to the same micrometer division as before. The distance through which the microscope has been moved is equal to a , the effective diameter of the stop.

The focal length, f , of the system is then found by any method—the goniometer method, described in § 4, or the revolving table method is convenient*—and the value of $n = f/a$ is calculated.

Method 2.—If a luminous point were placed at the focus F_2 (fig. 14), the rays from it, after passing through the system, would form a beam of rays parallel to the axis AF_2 . If a glass scale were placed against the mount of the lens, as ABC in fig. 14, the diameter of the beam of light could be at once read off, provided that the face of the scale which rests against the mount has a suitable matt surface, i.e., a surface which will so scatter any light which falls upon it that it is easy to distinguish the parts which are illuminated from the parts which are not illuminated.

* For a description of these methods see G. F. C. Searle, "Demonstration of Laboratory Apparatus and Experiments," *Proceedings of the Optical Convention*, 1912, p. 161.

The *divided* face of the glass scale should rest against the mount. The diameter of the bright patch on the scale is equal to the effective diameter of the stop.

The practical method of obtaining a beam of rays from a small area in the focal plane is to allow light from a flame to pass through a small opening in a thin metal plate placed in that plane. The source of light must, of course, be near enough to the plate to ensure that the stop is filled with light. But, since the rays do not now proceed from a *point*, the emergent beam will not be made up only of rays which are parallel to the axis and consequently the diameter of the bright patch on the scale ABC will no longer be equal to the effective diameter of the stop. The necessary correction is, however, easily found.

Let G be a point, in the plane of the diagram, on the edge of the circular opening in the plate in the focal plane, let N_2 be the nodal point of the lens system corresponding to F_2 , and let N_1 be the other nodal point. Then, if a ray from G is directed towards N_2 before it strikes the lens M, it will, on emergence from the lens L, proceed in the direction N_1K as if it came from N_1 , the two directions GN_2 and N_1K being parallel, by virtue of the property of the nodal points. Since G is in the focal plane, *all* the rays due to G which emerge from the lens L are parallel to N_1K . The extreme ray is the one which, in its passage through the system, passes through the point P on the edge of the stop, P and G both lying in the plane of the diagram. But Q_1 is the image of P formed by the lens L, and thus the extreme ray, on emerging from L, must be directed away from the point Q_1 . Hence, if Q_1CU be drawn parallel to GN_2 , it will give the direction of the extreme emergent ray. If C is the point in which it strikes the matt surface of the glass scale ABC, AC will be the radius of the bright patch on the scale.

Since CQ_1 is parallel to N_2G , the triangles

CBQ_1 , GF_2N_2 are similar, and hence $CB/BQ_1 = GF_2/F_2N_2$.

If d is the diameter of the opening in the focal plane, then $F_2G = \frac{1}{2}d$. If $BC = \frac{1}{2}b$ and $BQ_1 = x$, we have $b/x = d/f$ or

$$b = xd/f. \dots\dots\dots(3)$$

Hence, if c is the diameter of the bright circular patch on the matt surface of the scale,

$$c = a + b = a + xd/f, \dots\dots\dots(4)$$

and, therefore, $a = c - xd/f. \dots\dots\dots(5)$

The distance $BQ_1 = x$ is the distance between the matt surface of the scale and the plane of the image of the stop formed by the lens L . A plate of transparent glass of good quality is fixed against the mount of the lens in place of the glass scale (the latter may be used if the surface is cleaned) and a little lycopodium or other powder is placed on the face nearest to L . A microscope on a sliding carriage is then arranged so that its axis and the direction of motion of the carriage are parallel to the axis of the lens system, and the microscope is focused through the glass plate ABC first on the edge of the stop and then on the lycopodium, the adjustment being effected by sliding the carriage along its track. The distance through which the microscope is moved is equal to BQ_1 or x .

The diameter d of the hole in the plate may be measured by a glass scale placed with its divided face in contact with the plate; greater accuracy could be obtained by using a travelling microscope.

Method 3.—The determination of the distance BQ_1 may be avoided by using two plates with apertures of diameters d_1 and d_2 . Let c_1 and c_2 be the diameters of the corresponding bright patches on the matt surface of the glass scale. Since, by (5), $c - a$ is proportional to d , we have

$$(c_1 - a)/(c_2 - a) = d_1/d_2.$$

$$\begin{aligned}\text{Thus } a(d_1 - d_2) &= c_2 d_1 - c_1 d_2 \\ &= c_1(d_1 - d_2) - d_1(c_1 - c_2),\end{aligned}$$

and hence

$$a = c_1 - (c_1 - c_2) d_1 / (d_1 - d_2) \dots \dots (6)$$

A system for laboratory work.—A projection lens system such as is used in optical lanterns is convenient for the experiment. Such lenses are moderate in price (10s. upwards) and are reasonably well corrected. The cylindrical tube which forms the mount of the lens is, on account of its form, easily attached to a base. The stop may be inserted in the tube between the two components of the lens system. Fig. 15 shows the arrangement which has been found convenient at the Cavendish Laboratory. The pro-

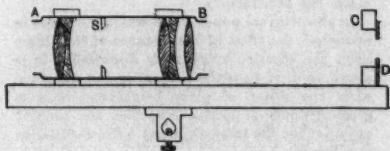


FIG. 15.

jection lens AB, of about 8 inches focal length, is mounted at one end of a base-board. At the other end of the board is fixed a wooden upright C with a circular opening. The metal plate D rests against that face of the upright which is furthest from the lens and is held in position by a rubber band or a spring. The lens is adjusted on the board so that the focal plane of the lens accurately coincides with the face of C against which D rests. The stop S should have a bevelled edge. A screw clamp fixed to the underside of the base-board allows the arrangement to be attached to an iron rod carried by a heavy base.

If the focal length is measured by the gonio-

meter method, a glass scale is substituted for the plate D, its divided face being in contact with the upright.

Though this arrangement is more convenient for daily laboratory work than an actual camera, students who possess cameras find the work more interesting if they investigate the stops on their own cameras.

A projection lens so mounted is a very useful piece of apparatus. If a cross-wire is fitted to the metal plate D, the arrangement acts as a good collimator. The spherical aberration will be small if the lens system is mounted on the board so that the end which is intended to face the lantern slide, when the lens is used for projection, faces the plate D. If the lens is mounted the wrong way round, its performance will be much less satisfactory.

In the original paper, from which the above is extracted, the effect of the distance of the object upon the effective aperture is discussed. It is shown that, if t_D is the proper time of exposure when the object, of given brightness, is at a great distance from the lens, then the time of exposure for the same object at a finite distance is given by

$$t = t_D (f + m \cdot O_1 F_1)^2 / f^2,$$

where m is the ratio of the linear dimensions of the image to those of the object. The point O_1 (fig. 14) is the centre of the diaphragm image T_1 , and F_1 is the focus corresponding to the nodal point N_1 .

(See "The Determination of the Effective Aperture of the Stop of a Photographic Lens." *Proc. Cambridge Phil. Soc.*, Vol. XVIII., p. 195.)

§ 8.—DETERMINATION OF THE REFRACTIVE INDEX OF A THIN LENS BY "FLARE SPOT" METHOD.

When light from a source of light S falls on a simple thin lens (spectacle lens) of focal length f , most of it passes through the lens and forms

an image of S. But some of the light suffers two reflexions within the lens, and this light gives rise to a second image of S of small intensity, the corresponding focal length, f_2 , being given by

$$f_2 = f(\mu - 1)/(3\mu - 1),$$

where μ is the refractive index. This secondary image is called a "flare spot." When f and f_2 have been found, the refractive index is given by

$$\mu - 1 = 2f_2/(f - 3f_2).$$

The measurements are easily made when a converging lens is used. A pair of cross-wires stretched across a small hole in a metal plate and illuminated by a *bright* flame serves as object. A pin is easily made to coincide with the secondary image of the wires; the secondary focal length is best found from the minimum distance between image and object, which is four times the required focal length. The ordinary focal length is found by any convenient method.

If two converging meniscus lenses are placed in contact with their concave surfaces facing each other, there are six secondary images, each of the corresponding focal lengths being positive. If a suitable converging lens is placed between the two meniscus lenses, there are 15 secondary images.

(See "Experiments Illustrating Flare Spots in Photography." Proc. Cambridge Phil. Soc., Vol. XVII, p. 205).

§ 9.—EXAMINATION OF IMAGES AS A TEST FOR ABERRATION.

On account of spherical aberration, a lens does not bring to a mathematical point all the rays which have reached it from an object point on its axis. When there is aberration, the emergent beam has at one place a finite minimum cross section called the least circle of aberration; the smaller this circle is the more

nearly is the lens free from spherical aberration for the given position of the object point.

A metal plate is pierced with three equally spaced circular holes, A, B, C, each about 0.1 cm. in diameter; the distance AC (from centre to centre) is about 2 cm. The holes are illuminated by a flame and their "images" formed by the lens system under test are received upon a very fine ground glass screen backed by a micrometer scale divided to 0.01 cm.; this scale is viewed by an eye-piece. This observing system is adjusted so that the luminous patch on the ground glass due to B is as small as possible.

Let d cm. be the diameter of the central patch in this case and k cm. the distance between the centres of the patches due to A and C. Then, if the distance between the centres of A and C is h cm., and if the diameter of the hole B, is b cm., the diameter of the "geometrical" image of B is bk/h cm. Then $d - bk/h$ is the diameter of the least circle of aberration for a point source at the centre of B.

The examination reveals at once whether the lens is free from spherical aberration or whether it is (1) under-corrected or (2) over-corrected for aberration. When there is no aberration, the patch is evenly illuminated in all positions of the screen. When the lens is under-corrected, so that the vertex of the caustic points away from the lens, a bright ring surrounding a faint circular patch is seen on the screen when the latter lies between the lens and the least circle of aberration. When the lens is over-corrected, the bright ring is further from the lens than the least circle of aberration.

If a plano-convex lens is tested with the holes A, B, C at a considerable distance from the lens, so that k/h is small, it will be found that the diameter of the least circle of aberration is much greater when the light falls first on the plane side than when the lens is reversed.

The test reveals any defect in the centering of a compound lens system. If the centres of

curvature of all the spherical surfaces do not lie on a straight line it will be impossible to obtain a circular patch upon the ground glass screen.

If a direct vision prism is placed between the eye and the micrometer the patch will be drawn out into a coloured band, which in the absence of chromatic aberration will have the same width throughout. When there is chromatic aberration, the red end of the coloured band can be made narrow by adjusting the screen; the blue end is then broad. By moving the screen, the blue end can be made narrow, but the red end is then broad. The motion of the screen indicates the amount of chromatic aberration.

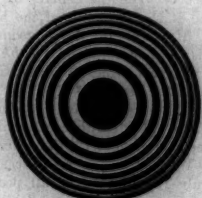


FIG. 16.

§ 10.—MEASUREMENT OF WAVE LENGTH BY ZONE PLATE.

A zone plate is formed by drawing on a white surface a series of concentric circles with radii proportional to the square roots of 1, 2, 3 . . . , blackening the alternate zones and taking a reduced photograph of this drawing (fig. 16) upon a glass plate with good surfaces. The alternate zones are thus opaque. Then light of wave length λ from a point on the axis of the zone plate at a distance u from the plate is concentrated to form an image at a point at a distance v , on the other side of the plate, where

$$1/u + 1/v = p \lambda n / r_n^2, \dots \dots \dots (1)$$

where r_n is the radius of the n th circle and p is one of the odd numbers 1, 3, 5 . . . and may be either positive or negative. By the construction of the zone plate, n/r_n^2 is constant. The zone plate thus acts as a thin lens of focal length f_p , where $f_p = r_n^2/p \lambda n$, but it has the peculiarity that has a series of both positive and negative focal lengths.

The diameters of a number of the circles on the zone plate are measured by a travelling microscope and the mean value of r_n^2/n is found.

Arrangements are then made for finding the focal lengths of the zone plate. A cross-wire or wire gauze, illuminated by sodium light, is used as object; its distance from the zone plate is u . The position of each of the images is found by making a pin coincide with it. The distance of the image corresponding to p from the zone plate on the *opposite* side to the object is v_p . If a telescope moving on an optical bench is available, the negative focal lengths can be found as well as the positive ones. The telescope is first adjusted so that the zone plate itself is in focus on the cross-wires of the telescope. The telescope is then moved along the bench so as to bring one of the images of the wire gauze into focus on the cross-wires. The distance between the two positions of the telescope is v_p . The observations are repeated for the other images.

The value of $1/u + 1/v_p$ or $1/f_p$ is then calculated for each image. According to the theory generally given, the focal lengths should be $f, f/3, f/5 \dots$, where f is the greatest focal length, but the "Phase reversal zone plates" in use at the Cavendish Laboratory, which were made by Prof. R. W. Wood, have the additional focal lengths $f/2, f/4, f/6 \dots$.

The mean value of pf_p is calculated and then λ is found from the equation.

$$\lambda = r_n^2/p f_p n^2 \dots \dots \dots (2)$$

In the arrangement exhibited to the Society, a parallel beam of light from a collimator fur-

nished with cross-wires was used. The distances of the images from the zone plate are then the corresponding focal lengths.

Zone plates are so useful for purposes of instruction that I regret that, as far as I know, no British optical manufacturer constructs them.

Discussion.

PROFESSOR SILVANUS THOMPSON said that to all those present who were or had been teachers of optics such a lecture as they had just listened to was most inspiring. It showed them how much better they ought to have done than they had done in the past in placing the subjects before their students. But they were by no means all in the stage either of students or of teachers of optics, and for those of them who were neither, there was a great deal that gave food for thought. Every one of the experiments raised questions which every optician had to face in one form or another, at one time or another. He had admired the extraordinary "ingeniosity" of many of these experiments. It took an able man and at the same time a man of very simple mind to imagine ingenious things of the kind which had been shown. They were an instructive proof how much better it was not to have at one's back a highly equipped laboratory with all modern appliances, because in such a case one was apt to rely upon appliances instead of taking things that came to hand and making use of them. They had had an object lesson that evening in the art of making use of simple appliances and materials. The clamps with geometrical slides to them were not nearly as well known or as much used as they should be. How many of them who had to fit up apparatus were still in the habit of using miserable things which were intended for a chemical laboratory—(laughter)—clamps that never fitted, slides that never slid, and very often screws that did not screw. One of the

things he had been going to criticise in the apparatus was that all the experience even of the lecturer was not adequate to getting a good clamp which would fit on at the first attempt. The screws were too smooth in the head to do their work. He thought they wanted hexagonal heads or something of that kind. The great use the lecturer made in his various demonstrations of the principal and nodal points of a lens system was another matter which ought to claim more than it did the attention and use of teachers of elementary optics. They ought to abandon once and for all, and never use again, the idea of a lens having but one optical centre. They might imagine it had, but it never had less than two, and might have four. That was to say, any real lens would have at least two principal points, and might have also two nodal points. That ought to be taught from the beginning; it was fundamental. Why then did they teach imaginary and futile principles without really laying down what was fundamental—that any real lens would necessarily have these two principal points and might have two nodal points? Among the ingenious things placed before them that night he had particularly admired the use of the direct-vision spectroscope for the study of spherical aberration. He wanted to suggest a further idea which probably had occurred to Dr. Searle—that if one wanted to teach people how to study chromatic aberrations, one should not begin with an ordinary lens, whether of flint or ground glass, and certainly not with an achromatic one, but should begin with a hyperchromatic one. It was a most useful thing in an optical laboratory to have two or three really fine, really good, *bad* lenses. (Laughter.) There was one other point which had nothing to do with what the lecturer had said, but it was a thing that he had had in his mind for a good many years, and which he might have mentioned before in the presence of some of them. That was the advantage there

was, in studying the problems of elementary optics, in regarding the ray as being a thing which was on the two sides of the lens an image of itself. If a light was placed in a certain position, P_1 , the image Q_1 of that point would be a point on a parallel line at a certain distance, but if the luminous point was put a little way from it at P_2 , the image would be at Q_2 , or, if it was put further down to another point, P_3 , the image Q_3 would be on the same straight line but nearer in. That was to say, so much of the light as started from the first point along the ray would enter the lens at one side along a line and would emerge at the other side along another line. In other words, a finite piece P_1P_3 of the ray might be regarded

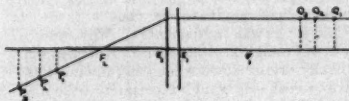


FIG. 1.

as having as its image a finite piece Q_1Q_3 at the other side. Again, if they took any lens system having two principal points and two principal foci, then the points which were at double the focal distance on either side would be symmetric points. If they took a point in the focal plane, its image would be at an infinite distance in the direction in which the light was travelling. The image of a row of telegraph posts situated at a distance greater than double the focal length of the lens, on one side, and stretching back to infinity, would be a row of telegraph posts of gradually diminishing size, inverted, and comprised between the symmetric point and the focal point on the other side of the lens as in fig. 2. Now the advantage of thinking of the two parts of the

ray as being a continuous thing was that the image of a bit of it would be another bit of it somewhere or other at a distance. This idea helped towards seeing at once certain solutions that otherwise might not be evident. Nothing was more common in examination papers on geometrical optics than to be given the size of an object in some particular position, and to be asked to find the size of its image; and when that question was asked it almost invariably meant the size *transversely*. The usual stock thing was an arrow drawn upright, and the real image on the other side would be an arrow upside down. Now the size transversely was, of course, a different thing from the size

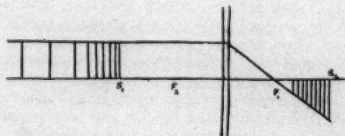


FIG. 2.

horizontally of the thing. One did not always want transverse sizes. How very seldom did they find this given as a question: Given an object of a definite size lying along the axis, find the image of it. The principle he had just laid down was that the ray on one side might be regarded as the image of the ray on the other side. Hence if the object were an arrow lying somewhere on the axis, the image of it would be a horizontal arrow lying somewhere on the axis further along. There was a very curious and interesting question as to where the image would be if the little arrow which was the object lay partly on the one side and partly on the other side of the principal focus; the image would be an arrow undoubtedly, but one half would be somewhere on the one side going to

infinity, while the other half would come back from infinity on the other side, one part being real and the other virtual. No doubt Dr. Searle had learnt nothing from those last remarks. (Laughter and applause.)

DR. ROSENHAIN wished to join Professor Thompson's admiration of the extremely ingenious and simple apparatus which Dr. Searle had shown. He (the speaker) was neither a student of optics nor, he was happy to say, a teacher of optics. He had come there for the purpose of getting a few "tips" about apparatus. He knew Dr. Searle's reputation, and he had not been disappointed. He thought the chemists had been slandered a little by having a very old-fashioned retort stand held out as an example of the kind of thing they habitually used in their laboratories. Well, he ventured to say in reply, on behalf of a much maligned class, that if some of these very pretty metal clamps were exposed to the fumes of a chemical laboratory, they would suffer even more than some of those rubber things which were sometimes used. No doubt, however, the geometrical clamps were extremely convenient.

Professor Thompson, carried away by his enthusiastic admiration for Dr Searle's apparatus and methods, had made a rather general remark as to how good it was not to have a well equipped laboratory behind one. He did not think that remark required a serious reply, or that Professor Thompson would care to have it applied too strictly and rigidly to the institution over which he himself presided. But he quite thought that for teaching purposes nothing was better than having apparatus of which one could see every part, and which to a large extent was improvised or improvisable; at the same time, when it came to doing systematic work, and doing it quickly and efficiently, that was quite irreconcilable with such rough-and-ready methods as were undoubtedly very admirable for teaching purposes. He was quite

sure that both Prof. Thompson and Dr. Searle would agree with him in that.

THE PRESIDENT, in moving a vote of thanks to Dr. Searle, said he had been reminded by the lecturer of some experiences of his own many years ago. An old friend of his, Dr. Czapski, of Jena, had come over to London to hear a mathematical demonstration by Lord Rayleigh. He (the speaker) had been very much surprised that he had come all that distance for the purpose of hearing a mathematical lecture. The Doctor's explanation, no doubt a perfectly honest one, was that it was a great æsthetic treat for him to listen to a mathematical demonstration by Lord Rayleigh. And he was quite sure that they would all agree with him that the demonstration by Dr. Searle which they had just witnessed had been an æsthetic treat. (Hear, hear.) Of course, it must be remembered that this was a demonstration of teaching apparatus, and he took it that the primary objects of such apparatus was to teach a student to use his hands and his brain, and to teach him the principles of the particular science concerned in the experimental demonstrations. Dr. Searle's apparatus did not profess, he was sure, to be in any way apparatus suitable for commercially making the various tests described. He had been very much amused by Dr. Searle's reference to the many focal lengths which even the most simple lens might be possessed of. That reminded him of a story. For many years after the telescope was first used by Galileo, the great test of a very fine telescope was made on Jupiter. If the satellites were clearly visible, the telescope was accepted as a very good one. Some time ago a gentleman wrote to one of the daily papers to say that he had a most wonderful mirror in his possession, an heirloom which had been handed down for generations in his family. He went on to say, "When I go outside and look at Jupiter, I cannot see the satellites of Jupiter, but if I take this wonderful

mirror that has been in my family for so many years, and hold it horizontally, and look at the reflection of Jupiter, then I can see not only Jupiter, but I can see its satellites." Well, a gentleman who knew something about it wrote to correct the first correspondent, and pointed out that it was a case of multiple reflections; but the value of the explanation was altogether wasted, because unfortunately some wag got in first with a further statement that he had an even more wonderful mirror than that of the gentleman who had written the first letter. This third gentleman said: "With my mirror, when I repeat the experiment the gentleman describes, and rotate the mirror in its own plane, not only do I see the satellites but I see them revolving around the planet." (Laughter) He felt with Prof. Thompson that the apparatus which had been exhibited that night was a wonderful example of what could be done with the most simple apparatus when brains and hands were used in putting it together. He had been particularly struck with the charm of the little apparatus known as the goniometer, which no doubt could be quite simply used and be adapted for many purposes in the optical laboratory.

SOME NOTES REGARDING A CRYSTAL
ENCLOSED IN THE SUBSTANCE OF A
PIECE OF OPTICAL FLINT GLASS.*

By MR. JAMES WEIR FRENCH, B.Sc.

Read 25th May, 1916.

THE figure shows a micro-photograph of the crystal magnified 400 times.

The following are the principal features observed:—

(1) The crystal was found entirely enclosed in a piece of optical flint glass which had been moulded when in a plastic condition into a flat plate form.

(2) The plane of the crystal was parallel to the surface of the plate, as is evidenced by the fact that the crystal is in focus over its whole area.

(3) No other crystals were found in the piece of glass.

(4) The form of the crystal is hexagonal.

(5) The inner portion shows no sign of crystallisation with the exception of some markings at the centre.

(6) When the crystal is viewed very obliquely there is some uniform reflection from the clear central portion, indicating a difference in density of the material. At other obliquities the centre is as transparent as the other portions of the unaffected glass and it is only the crystallised portions that then reflect light.

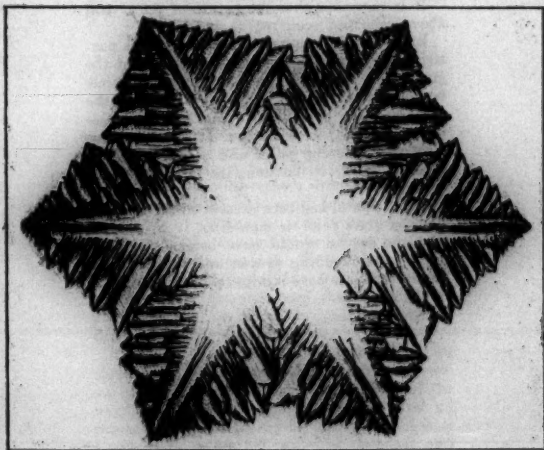
(7) The stream lines radiating from the points of the crystal actually exist. The other marks on the photograph are camera defects.

(8) At low magnifications there are no observable colours.

* Communicated by the President, Mr. Frederic J. Cheshire, F.R.M.S.

(9) When magnified 100-fold and over, the ribs of the crystal appear green when suitably illuminated. The colour of the green corresponds with the colour of the edges of glass rounded by surface tension.

(10) When magnified 1,000-fold the facets of the crystal appear bright green and bright red.



(11) When viewed by polarised light there are no observable polarisation colours, which suggests that the crystal occupies a very thin layer.

(12) The source and actual constitution of the glass are not known.

The crystal is probably an unusually perfect example of the devitrification of glass. When glass is maintained at a temperature which need

not be above the softening point, devitrification of the glass occurs and crystals are formed.

Large quantities of crystalline masses are obtainable from the floor and walls of the glass tanks in which the glass is kept molten for long periods in the manufacture of sheet glass. Devitrified glass of this kind is sometimes technically known as "crystalline dog."

It is suggested that the growth of the crystal above described occurred as follows:—

In the original lump of glass there was a particle rich in some particular constituent of the glass. In the moulding process this particle became flattened into a layer parallel to the surface of the glass plate. The moulded glass was maintained for some time at a temperature near the softening point and under these circumstances devitrification in the spot layer occurred.

If the crystal had been present in the original lump of glass prior to moulding, it is probable that the crystal would have become deformed during the moulding process and that it would not in that case have lain parallel to the surface of the plate.

SANDS FOR GLASS-MAKING, WITH ESPECIAL REFERENCE TO OPTICAL GLASS.

By P. G. H. BOSWELL, A.R.C.S., D.I.C., D.Sc.,
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Read 25th May, 1916.

I.—Introduction.

OF the manifold uses to which sand is put none is more important than that for the making of glass. In spite of this fact, most glass manufacturers, like others interested in industries where suitable sand is essential, have done little or nothing towards investigating the reasons for the suitability or otherwise of the sandy materials they use. Economic geologists are equally culpable in not having investigated the resources and characters of the abundant and varied sands, and also related deposits, of this country.

The term "sand" in its popular sense includes many mixed products which are ruled out in the more restricted geological use of the word. The geologist understands by sand an incoherent rock made up of individual mineral grains with a limited range of chemical and mineral composition, but most importantly, having the majority of its grains, which are usually somewhat rounded, lying between certain size-limits. The mean diameter of the grains of a true sand does not rise above 2 mm., or fall below $\frac{1}{16}$ mm. Thus a "very coarse sand" has a high percentage by weight (ideally in each case, 100 per cent.) of grains of diameter 2 mm. to 1 mm., a "coarse sand" of diameter 1 mm. to $\frac{1}{2}$ mm., a "medium sand"

of diameter $\frac{1}{16}$ mm. to $\frac{1}{8}$ mm., a "fine sand" of diameter $\frac{1}{8}$ mm. to $\frac{1}{4}$ mm., and a "superfine sand" of diameter $\frac{1}{4}$ to $\frac{1}{2}$ mm. Each of these size-limits is known as a "grade," and the separation of a deposit into its constituent grades is known as "grading."

Commercially the term "sand" includes materials of varying grades (as for example, moulding "sands") and differing composition; it is also applied to the products obtained by crushing consolidated sandstones and quartzites.

Most sands consist largely of silica occurring as broken fragments of the mineral quartz. The impurities present may consist of grains of other minerals, chemical substances tending to coat and cement the grains together, or as dust, etc., mixed with the deposit.

Upon the varying chemical, mineral, and grade compositions, as well as upon the shape of the grains, the commercial application of sands depends.

Before discussing the particular characters required of sands used in glass-making, a few only of the many uses of sands in industry may be quoted. The farmer lightens his heavy clay soils by the addition of sand, thereby rendering the land more permeable to air, gases and water, warmer, more amenable to working and more easily drained. It is the addition of coarse grade material which is necessary here. If the sands are calcareous, the effect is heightened by the flocculation of the clay particles.

Since sands are usually made up of hard and tough materials, frequently as angular fragments, they are of considerable value as abrasives. Formerly their employment in this connection was almost limited to the housewife's scouring of kitchen utensils, etc., but whilst fine sands are still similarly used as the basis of scouring soaps, the properties are more extensively utilised in the sand-blast applied to cutting, grinding and etching glass, cleaning metal-castings, arming stone-saws, and so forth.

In the process the sand loses its sharp edges and requires periodic renewal.

Closely related are friction-sands, employed not only on polished roads and in frosty weather to reduce slipping, but used frequently to increase the grip of car-wheels upon metal rails. Sands used in this way, besides being hard, tough, and angular, must also be dry and of even grade to slip freely down the feeding funnel. The use of sands in hour-glasses and egg-timers, though not of great commercial importance, depends upon the same properties. A well known geologist when met by the question as to the geological horizon and properties of the sand used in hour-glasses, avoided a confession of ignorance by remarking that it was not the custom to measure geological time with hour-glasses!

Besides being largely employed for building purposes, where angular, fairly coarse sands are mixed with lime to make mortar, sands are mixed with the stiff clays in brick-making in order to reduce the shrinkage and tendency to cracking when the bricks are burnt.

From the point of view of public health, the employment of sands for water-filtration involves important considerations. The sands so used should be well graded, often fairly coarse, should not contain clayey or organic matter, or any amount of lime.

REFRACTORY SANDS.

But by far the most important industrial uses of sand depend upon its power to withstand high temperatures. It is our commonest and most abundant refractory material, and is, therefore, largely used in metallurgical practice. Its silica percentage must then be high, and that of the alkalis and alkaline earths low. The linings, floors, sides, and roofs of kilns and furnaces require highly quartzose sands and sandstones, and the moulds in which the molten metal is cast must similarly be made of highly

refractory "sands" of peculiar grade composition, which will enable them to bind well and stand up against the metal, but will allow ventilation of the casting.

The importance of the question of investigating sands used for glass-making will be realised hereafter. Other commercial applications need not be detailed, but it is interesting to note that just as "flint-glass" is not now made from flint but from sand, so "sand-paper" is often not made from sand, but from powdered glass!

Many uses of sand, for example, in the making of our hour-glasses, as an absorbent for ink before blotting paper was introduced, for the sanding of stone and brick floors, and others, have almost, if not entirely, disappeared.

Just as, chemically, dirt has been defined as "matter in the wrong place," sand may in certain industries be inimical. Among others the agriculturist and traveller fully recognise that it is possible to have too much sand, yet its value around our shores, apart from the commercial aspect, is both æsthetic and protective, and there are few who would join with the Walrus and the Carpenter in weeping over the quantity and sympathize with them when "If this were only cleared away," they said, "it *would* be grand!"

II.—Sands for Glass-making.

A.—IMPORTANCE OF SANDS IN THE PROCESS.

The choice of a suitable sand for use in the making of glass, particularly such special glasses as those for optical purposes, is an important question. Upon the quality or purity of the sand depend the transparency, brilliancy, lustre, and hardness, of the glass, while its uniform density is determined to no small extent by the mechanical composition of the sand. Due regard must be paid to obtaining and treating suitably the sand in order that the "life" and "water-whiteness" of the best glass may be

obtained. For optical glass and best "crystal" only the purest sands can be used.

In the matter of bulk also, the sand is the most important ingredient, constituting 52 to 65 per cent. by weight of the "batch," or mixture of raw materials. The finished product, glass, contains 60 to 75 per cent. of silica, the increase in proportion being due sometimes to the fact that silica is added as fclapar to the batch, but usually owing to the loss of gases, such as water, carbon dioxide, sulphur dioxide, etc., during the melting process.

B.—NATURE OF SANDS.

Sands have been produced by the natural breaking down of the rocks which build up the earth's crust. The geological agencies of heat and cold, frost, and rain, are concerned in this gradual shattering of the rocks by chemical and mechanical means. Running water and gravity are continually at work moving the disintegrated material down to lower levels, and eventually to the sea.

The less stable minerals of the rocks decompose and are broken down into soluble salts and fine-grained insoluble residues, whilst the obstinate ones, such as quartz, are loosened from one another. Material of very mixed chemical, mineral, and grade composition, is thus produced, but the sorting action of wind and water soon comes into play. This sorting is controlled by the size and weight of the mineral fragments, the larger and heavier grains soon being dropped, while the fine clayey material is often carried for long distances. While the grading and the sorting of minerals according to density is never perfect, a strong tendency always exists towards simplification. The sediment, in time, comes to consist almost entirely of one mineral and of equal-sized grains.

From the point of view of the production of sands suitable for glass-making, the last factor is an extremely important one. Considering

purity from the standpoint of silica percentage, pure sands are not common in occurrence, nor abundant when found. Great variation occurs in the mechanical (or grade) composition of sediments, and well-graded sands are by no means so frequently met with as we might expect. It is an interesting fact that all the best glass-sands i.e., the purest and best-graded deposits, occur comparatively late in the geological time-scale.

The minerals composing sands may be divided into two groups, the allogenic minerals, derived from older rocks, and the authigenic minerals, which were formed at the time the rocks were deposited or subsequently. We are mainly concerned with the allogenic minerals. Some of the heavy detrital minerals are fixed chemical compounds, others are molecular mixtures which vary somewhat in their composition. Among the latter are such silicates as the pyroxenes, amphiboles, olivines, epidotes, etc.

C.—METHODS OF INVESTIGATION.

For the complete understanding of the characters and properties, and hence the economic uses and value of unconsolidated sediments, we need to know the chemical, mechanical, and mineral composition, and the corresponding three analyses of each sample.

Chemical analyses are carried out by methods familiar to all, the usual procedure in dealing with rocks being followed.¹ Mineral analyses are made in detail by treating the sediment with heavy liquids of densities ranging from 2.56 to 3.3. The minerals such as quartz and feldspar, which usually constitute 98 per cent. or more of a sand, have a density below 2.8, while the iron oxides, ferro-magnesian minerals such as biotite mica, hornblende, augite, epidote, garnets, staurolite, and other heavy minerals (alu-

¹ See for example W. F. Hillebrand. U.S.G.S. Bull. 176 (1900). H. S. Washington. "Chemical Analyses of Rocks." 1906. New York.

minous silicates such as muscovite mica, andalusite, kyanite, sillimanite, topaz, etc., and also zircon, rutile, etc.) have a density greater than 2.8. Bromoform, mercury potassium iodide solution, cadmium borotungstate solution, and other liquids of density about 2.8, are therefore employed to separate off the heavy crop of minerals which sink in them, from the lighter and much bulkier crop of quartz, etc., which floats. The crop of density, 2.8, usually does not reach over 1 per cent. by weight, but it may range from 0.01 to 4 or 5 per cent. Other methods are employed, including microscopic examination, for the separation and investigation of the individual mineral grains.²

Examination of a glass-sand under the microscope is easily carried out and yields interesting information. A small quantity of the sand may be mounted temporarily in a drop of clove oil upon a glass slip. Owing to the fact that the mean refractive indices of quartz and clove oil are very close to one another, the quartz grains, if they are clean, are almost invisible in ordinary light. A very thin pellicle of clayey or ferruginous matter upon them renders them easily visible, and the extent of iron-staining is thus quickly observed. Felspar, when fresh and unaltered, is similarly only faintly outlined, but when "kaolinised," i.e., decomposed into micaceous and clayey matter, appears dusky. Such an excellent glass-sand as that from Fontainebleau almost disappears on immersion in clove oil. Heavy detrital grains, when they do occur, usually stand out in bold relief, owing to their higher refractive indices.

The mechanical analysis is carried out with a view to finding the sizes of the constituent grains, and the percentage by weight of each grade. The grades in common use are:—

² For details of these methods, etc., see T. Crook in Appendix to Hatch & Rastall "Petrology of Sedimentary Rocks." London, 1913.

Greater than 2 mm. diameter ...				Gravel Grade.		Sand Grade.
1 mm. and less than 2 mm. diam.				Very coarse sand	Coarse sand	
1				Coarse sand	Medium sand	
$\frac{1}{2}$				Medium sand	Fine sand	
$\frac{1}{4}$				Fine sand	Superfine sand or Coarse silt	
Less than $\frac{1}{8}$				Silt	Clay Grade.	Silt Grade.
Greater than 2 mm. diameter ...	...	...	...	...	...	
" 1 mm. and less than 2 mm. diam.	1	"	"	...	...	
" $\frac{1}{2}$	"	"	"	...	...	
" $\frac{1}{4}$	"	"	"	...	...	
" $\frac{1}{8}$	"	"	"	...	...	
" $\frac{1}{16}$	"	"	"	...	...	
" $\frac{1}{32}$	"	"	"	...	...	
" Less than $\frac{1}{8}$	"	"	"	...	...	

The oldest and roughest method of making mechanical separations is the well-known one of sifting. The method is, however, inaccurate for scientific work, and if carried out at all, separations should be made with brass or copper sieves having punched round holes of diameter 2 mm., 1 mm. and $\frac{1}{2}$ mm. Below $\frac{1}{2}$ mm. punched holes are not practicable, and squared-holed wire-mesh screens must be used. In all cases it is the mean diameter of the sand-grains which determines what passes the sieves, but in the square mesh sieves the grading is less accurate owing to the greater length of the diagonal of the aperture. Sifting is thus not to be recommended below $\frac{1}{2}$ mm.; separation of smaller grades may be made to a fair degree of accuracy by the use of elutriation. For detailed descriptions of the processes of elutriation in air or water, and the various forms of apparatus devised, reference must be made to the works cited.¹ Briefly put, elutriation depends upon the power of upward currents of air or water of varying velocities to buoy up grains of minerals of different surface areas. It has been found as a result of both calculation and experiment that the final velocity acquired by a small fragment of solid, approximately spherical, when falling freely in a fluid, such as water, depends primarily upon its surface area (and, therefore, upon its mean diameter) and to a less extent upon its density. Subsidence methods have thus been used (and are used in industry

¹ Schoene, E. "Ueber Schlämmanalyse." Berlin, 1876.

Keilhack, K. "Lehrbuch der praktische Geologie." Leipzig, 4th ed., 1908.

Crook, T. Proc. Roy. Dublin Soc. (Econ.) Vol. I.; pt. 5 (1904), p. 267, and *op. cit.*, p. 349.

Stadler, H. "Grading Analyses by Elutriation." Trans. Inst. Mining and Metall. XX. 1912-13, p. 686.

to-day, e.g., in the preparation of china-clay) to separate sediments into various grades. Such methods are not scientifically accurate, hence the adoption of elutriation, where upward currents of fluid are utilised, equal to or just in excess of the respective falling velocities of the particles of definite grade sizes which it is desired to separate. Water is generally used, and an elutriator consists essentially of one or more cylindrical vessels of known area of cross-section. The velocity of the upward current of water passing through is controlled by the head of water outside (often adjustable) the diameter of the vessels, and the size of the outlet jet. Usually a manometer-tube is attached to the elutriating cylinder to register the pressure and thus indicate the velocity.

A weighed quantity of sediment is placed in the cylindrical vessel, and by the variation in the velocity of the water-current, grading is effected, the respectively smaller grades being carried over, collected, dried and weighed. The various grades are calculated to percentage weights and the results may be expressed graphically.

The apparatus is easily constructed, being made of glass, and should find a place in the laboratory of every glassworks.

III.—Desiderata of Glass-Sands.

A.—CHEMICAL COMPOSITION.

The chemical analysis of a glass-sand should indicate the percentage of silica (SiO_2), alumina (Al_2O_3), iron oxide (expressed as Fe_2O_3), and loss on ignition (H_2O , etc.). It is desirable in sands generally to have a full analysis, but those proposed for use in glass-making should bear little more than a trace of alkalis, lime, magnesia, etc. The silica percentage should be very high, preferably over 98 per cent., which means that the sand must be a pure quartzose deposit. For common bottle-glass the percentage may drop to 95, and for the best quality glass

such as that known as "crystal," used for table-ware, and optical glass, the sand should possess 99.5 per cent. or more of silica. Alumina is sometimes desirable in a sand, particularly when it is to be used for the manufacture of refractory ware such as thermometer-glass. Hovestadt mentions¹ that thermometer-glass made in the Thuringian Forest district owes its special quality to certain sand found only in the neighbourhood of the village of Martinsroda. The glass stands repeated melting, blowing and fusing, without change, while ordinary glass, such as that of windows, becomes rough and dull of surface even after short exposure to the flame. The cause of excellence is believed to be due to the 3.66 per cent. of alumina present. In sands free from iron the alumina usually occurs as the mineral feldspar. Its presence as clay (hydrated aluminium silicate), either free or as a dust coating quartz-grains, is highly undesirable.

Unfortunately, alumina in a sand usually carries with it other impurities, especially iron and lime, so that it is advisable to use a pure quartzose sand for glass-making and to add the alumina required as such to the batch which is melted up to make glass.

Iron compounds are undesirable in a glass-sand, whether they occur as hematite and limonite coating the quartz, or as detrital mineral grains of magnetite, titanoferrite, or the ferromagnesian minerals enumerated above. The effect is to give the glass a pale-green or yellow colour. By the addition of decolorisers such as manganese dioxide, arsenious oxide, etc., a certain amount of correction may be obtained, and glass practically white or with an exceedingly faint pink tinge be yielded. For the best glass, including optical glass, the iron percentage (estimated as Fe_2O_3) should not exceed 0.05 per cent., but for window and plate-

¹ "Jena Glass," translated by J. D. & A. Everett. MM. & Co., London, p. 21.

glass, chemical apparatus, globes, etc., 0.1 to 0.2 or even 0.3 per cent. may be permitted. Correction for iron by means of decolorisers is inadvisable in the case of optical glass on account of the absorption of light owing to the neutral tint produced.

Sands containing over 99 per cent. of silica and less than 1 per cent. of iron and alumina rarely carry more than a trace of lime, magnesia, titanium, potash, soda, etc. Sands containing any quantities of these compounds are not suitable for glass-making.

B.—MINERAL COMPOSITION.

Just in the same way as valuable mineral products, such as gold, tin, and gems, are concentrated by natural agencies in stream-sands as a result of their high density, and are further concentrated by panning at the hands of the prospector, so the heavy detrital minerals of a sand may vary in proportion to the lighter quartz and felspar, according to the conditions of deposition. Proximity to the source of the heavy minerals, and oscillatory current-action after or during deposition, tend to increase the amount. Taking an average for British sands and sandstones of all geological ages, the percentage weight of mineral grains of density greater than 2.8 is probably about 0.5. In certain very pure sands, sandstones and quartzites, it falls as low as 0.01 per cent.; occasionally it rises in other deposits to 4 or 5 per cent. If a sand is used for glass-making, the percentage of heavy minerals should be low, since iron oxides and ferromagnesian silicates constitute a large proportion of the dense minerals present.

It is now possible to recognise many sands both as regards geological age and locality, from their heavy mineral assemblages. A considerable number of minerals, with much variety of form, size, colour, inclusions and degree of alteration, in their grains, may be met with in

these assemblages, and detailed examination will in most cases serve to indicate whether successive consignments of a sand come from the same bed or pit. Glass-manufacturers, in using the methods of mineral analysis, have in their hands a valuable check on such foreign deposits as Belgian and Dutch sands.

C.—MECHANICAL COMPOSITION.

A perfectly-graded ideal sand would have all its grains of closely the same size, that is, 100 per cent. by weight would lie between two close limits of size (as defined on p. 227).

No natural sands are perfectly graded, but those which most nearly approach the ideal are shore-sands and dune-sands which have been continually winnowed by wind and washed by water. Smaller grains have all been carried far away, and larger ones left behind when the deposits themselves have travelled along the shore. Dune-sands and sea-sands around our coasts to-day, and those formed under similar conditions in past geological ages, may have as large a proportion as 97 per cent. of their grains of one grade size, usually "medium sand" of diameter between $\frac{1}{4}$ and $\frac{1}{2}$ mm.

By subjecting the sands which have proved most successful in glass-making (as well as those which have not) to mechanical analysis, it has been found that the grade is an exceedingly important factor. A few glass-manufacturers who have studied the technique of their work have realised this and placed the question of size of grain on an equality with that of chemical composition. Nevertheless, no systematic work seems to have been done, but it is to be hoped by now that elutriators have been set up in the laboratories of a few glass-works.

In order that the batch containing the sand, alkalies, and other compounds may melt freely, evenly, and as rapidly as possible, at least 70 per cent. of the sand should be of one grade, preferably medium sand of diameter $\frac{1}{4}$ to $\frac{1}{2}$ mm.

The practice in the United States glass industry appears to consist in the use of graded deposits of rather coarser grain than in this country. The presence of coarser fragments than the average in a glass-sand results in these being left unmelted as "stones" while the rest of the batch has become molten "metal." If heating is continued until the "stones" are also dissolved, some glasses are spoilt by the chemical changes which take place, and the fresh gases which are evolved.

Finer material belonging to the clay grade (diameter less than .01 mm.) or silt grade (diameter between .01 and .05 or possibly .1 mm.), whether occurring as thin pellicles on the larger quartz grains, or acting as a bind, is inimical from the glass-making point of view. Clayey material, especially if it is such a pure clay as kaolin or china-clay, is refractory, and clouds the glass. Fine particles also carry with them air-bubbles, from which it is very difficult to clear the metal. If the batch is melted in pot-furnaces, the finer quartzose material (superfine sand, silt, etc.) tends to melt first and sink to the bottom. Subsequent melting of the rest of the batch results in the production of a pot of metal of varying chemical composition and density according to depth, as usually no arrangements are made for stirring the mixture. The variation in density of the metal results in the production of objectionable "wavy" and "cordy" glass upon blowing.

Tank Furnaces.

When tank-furnaces are used, the strong blast of air and gas which plays through the "ports" in the side walls over the bath of metal, carries off the fine material of the sand, and a real loss in bulk occurs, as well as a change in chemical composition of the remainder. The other powdery material of the batch such as soda-ash (Na_2CO_3) melts too rapidly to be lost, but the fine silica dust is more refractory and "blowing-

out" results before it can be melted. The statement that in America glass-making, sand of diameter less than $\frac{1}{80}$ inch burns out in the batch¹ is not borne out by British and Continental practice, where sands of diameter down to $\frac{1}{4}$ or even $\frac{1}{10}$ mm. ($\frac{1}{100}$ to $\frac{1}{150}$ inch) are continually in use. Fine sand of diameter $\frac{1}{4}$ to $\frac{1}{10}$ mm. diameter does not appear to be objectionable—rather the reverse, but it is not easy to find pure and well graded deposits of this character. The important point is that for most glasses, the melting of the batch should be rapid and even.

D.—SHAPE OF GRAIN.

Some disagreement exists as to how far the shape of the grains constituting a sand is of importance in glass-making. Most manufacturers, if they have a preference, lean towards the use of an angular or sub-angular sand. However, sands composed of rounded grains are successfully used for glass-making in the United States.

The most obvious reason for the preference for angular grains seems to be that the grains fuse more rapidly in the batch, the melting proceeding at the corners and edges of the grain. The tendency of the batch to pass freely, quickly and evenly into metal is thus increased.

These remarks do not apply altogether to the making of optical glass, where no danger of blowing out exists, a long period of melting is required, and stirring is always adopted.

E.—OTHER CONSIDERATIONS.

It is necessary that a glass-sand should be dry, for moisture present tends to form bubbles or otherwise spoil the glass or cause variation in its properties (*e.g.* in optical glass). For all special work, the sand, or batch including it, is dried in large ovens.

¹ United States Geological Survey Bull. 285 (1906), p. 454.

Freedom from organic matter should also be ensured, for it usually has a reducing action which is not always desired in glass-making (though coke is sometimes added for that purpose in making common glass, as, for example, where salt-cake is used). Organic matter is burnt out with the water during the roasting in the ovens.

Water and organic matter appear in the chemical analyses under "loss by ignition."

The ideal sand for making the best glass is thus one with practically 100 per cent. silica free from all but the slightest trace of iron, and composed of angular grains of quartz (but not other forms of silica as well) all of the same size. The sand should also be dry and cleared from dust or any clayey coating on the grains. Although not so essential, it is desirable not to have organic matter present.

IV.—British and Foreign Glass-sands.

In the previous section the importance of the information yielded by analyses of glass-sands has been emphasised, and the criteria of a good sand stated. Before passing on to discuss British sands which might satisfy the requirements of the maker of optical glass, it will be instructive to examine in detail the properties of the sand from Fontainebleau which has been so largely used for the purpose in this country. The well-known sand from Lippe, used so extensively in the German industry, will also be described. We may then hope to realise the desiderata of a first-class glass-sand, and know what to look for in any other deposits exploited for glass purposes.

1. FONTAINEBLEAU SAND.

The sand, of Upper Oligocene age (Sables de Fontainebleau, Stampian Stage of the Oligocene), is associated with lignites.¹ To the un-

¹ Revision de la Feuille de Fontainebleau. Bull. Carte Géol. France. No. 122. Vol. 19. (1908-9), p. 7.

aided eye, the sand is a beautiful white, fine, even deposit. All glass-sands should be examined in the field with a hand lens. With such a lens the sand appears to be composed of water-clear quartz, and only here and there a few dark grains are to be seen. Unless the sand has been washed after its arrival at the works (and for all special glasses it is usually washed carefully), a little dirt and occasional specks of black coaly matter may be seen. Such impurities are, of course, adventitious and obtained from trucks and barges in course of transit. The grains have no coating of white clayey matter as many sands have, their white colour being due to the reflection of light from irregular surfaces. They are almost invisible under the microscope when immersed in media such as clove oil, Canada balsam, etc., the refractive indices of which lie very near to that of quartz. The grains are free from the pale yellowish or brownish coating of iron oxide so frequently seen in sands.

CHEMICAL COMPOSITION.

Chemical analyses show that the sand contains 99.65 to 99.97 per cent. of silica, and, therefore, must be composed almost entirely of pure quartz. Only a small quantity of iron oxide is present, hence glass made from the sand is practically water-clear (provided no colouring compounds are added). The alumina percentage is very low, being probably accounted for in the rare grain or two of feldspar seen, and in the few heavy minerals present. Lime and magnesia are practically absent.

CHEMICAL ANALYSIS OF FONTAINEBLEAU SAND.

SiO ₂	...	...	99.65 per cent.
Al ₂ O ₃	...	...	.21 "
Fe ₂ O ₃	...	...	.04 "
Loss on Ignition	...	...	.16 "
100.06 per cent.			

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MINERAL COMPOSITION.

Fontainebleau sand, like many other pure quartzose sands, contains less than 0.1 per cent. of heavy minerals, which is much below the average for all sands. In this heavy crop, of density > 2.8 , the minerals present are clean and fairly coarse in grain. They include magnetite, zircon, rutile, ilmenite, rounded tourmaline in blue and brown grains, staurolite, andalusite, muscovite, kyanite, limonite, etc.

From the chemical compositions of these detrital minerals, it is seen that silica is present in some amount, and that small quantities of iron, aluminium, titanium, magnesium, zirconium and boron occur. The proportion of heavy minerals in the sand is so small that even with the large bulk of sand used for the batch, the aggregate amount of impurities is slight.

MECHANICAL COMPOSITION.

The evenness in grain of a sand (to the eye) has been commented upon, and mechanical analyses determined by elutriation give the following results:—

	$> \frac{1}{2} \text{ \AA} < 1 \text{ mm.}$	$> \frac{1}{2} \text{ \AA} < \frac{1}{2}$	$> \frac{1}{16} \text{ \AA} < \frac{1}{2}$
1.	$< 0.1\%$	70.6%	28.6%
2.	—	70.3	28.3

	$> \frac{1}{16} \text{ \AA} < \frac{1}{16}$	$< \frac{1}{16} \text{ mm.}$	Total sand-grade $> \frac{1}{16} \text{ \AA} < 1 \text{ mm.}$
1.	2.8%	—	97.2%
2.	0.6	0.8	98.6

1. Obtained by the kindness of Mr. Frank Wood, Managing Director of Messrs. Wood Bros., glass manufacturers, Barnaley.

2. Obtained by the kindness of Mr. G. Jenkinson, Managing Director of the Edinburgh and Leith Glass Co., Ltd., of Edinburgh.

Lastly as regards shape of grains, Fontainebleau sand is made up of grains consistently sub-angular. To what extent the rapid and even melting of the batch depends upon the shape of the sand-grains, is a disputed question, but it is clear that such a sand as that of Fontainebleau, the grains of which are all of similar shape, is preferable to one containing a mixture of angular, sub-angular, and rounded grains.

ECONOMICS.

In addition to the recommendation afforded by chemical purity, evenness of grade, and suitability of size and shape, Fontainebleau sand can be transported cheaply and conveniently to most British glass-making districts. This is partly due to the insular position of Britain, and partly to the fact that the sand can be directly shipped from Rouen, and, without further handling, may be discharged high up the estuaries which notch our industrial areas. In pre-war times it was delivered in Britain at a fairly cheap rate (12s. to 23s. per ton). Owing to the shortage of labour and lack of barges, the price has since risen to as much as 50s. to 60s. per ton.

2. LIPPE SAND.

Two different samples of this famous glass-sand from Dörentrup, Saxony, were subjected to analyses and examination through the kindness of Dr. Walter Rosenhain, Director of the Metallurgical Department, National Physical Laboratory, Teddington.

The sand occurs in deposits of Miocene age, and is associated with rafts of brown coal.¹

In this connection it may also be noted that the valuable glass-sand of Hohenbocka, in

¹ Dritt. Jahresber. des Niedersachs. Geolog. Vereins. 1910, p. 185, also see p. 171, and Zeitschr. der Deutsch Geol. Gesell. Band XL, 1888, p. 310.

Prussia, occurring in Miocene beds, is also associated with carbonaceous layers.¹

Both samples of Lippe sand are beautifully white, of better colour even than that from Fontainebleau. The better sample is finer in grade and remarkably even, while the second is rather coarser. The chemical analyses are as follows:—

	I.	II.
	Per cent.	Per cent.
Silica	99.88	99.73
Al ₂ O ₃	.18	.20
Fe ₂ O ₃	n.d.	n.d.*
Loss on Ignition	.21	.23
	100.27	100.16

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* Separate determinations of Fe₂O₃ gave I. .02 per cent. and II. .03 per cent.

It is said that, such is the remarkable constancy of chemical purity, the sand is guaranteed when sold to contain 99.98 per cent. of silica. The mechanical analysis of the finer and better sample is

> $\frac{1}{16}$ and < 1 mm.	> $\frac{1}{16}$ and < $\frac{1}{8}$	> $\frac{1}{16}$ and < $\frac{1}{8}$
One or two grains.	78.6%	19.9%
> $\frac{1}{16}$ and < $\frac{1}{8}$	< $\frac{1}{16}$ mm.	Total sand-grade > $\frac{1}{16}$ and < 1 mm.
0.4%	1.1%	98.5%

Examination and measurement under the microscope show that the average diameter of the

¹ K. Keilhack, Jahrb. König. Preuss. Geol. Land. 1908. Bd. 29, pt. II., p. 214.

grains is .3 mm. (or perhaps slightly less). They are composed, with the exception of the few detrital minerals, of colourless, clean quartz, sub-angular to angular in shape. The mechanical analysis of the second sample is

$> \frac{1}{16}$ mm.	$> \frac{1}{16}$ and $< \frac{1}{8}$	$> \frac{1}{16}$ and $< \frac{1}{4}$
10.2%	83.8%	1.4%
$> \frac{1}{16}$ and $< \frac{1}{8}$	$< \frac{1}{16}$	Total sand-grade $> \frac{1}{16}$ and < 1 mm.
1.0%	1.6%	97.4%

The grains, which are sub-angular, are similar to those of I. under the microscope, but are, for the most part, nearly $\frac{1}{8}$ mm. in diameter.

The heavy residue of minerals is small in each case (less than 0.01 per cent. by weight), but, as is often the case, was greater in the finer sand than in the coarser.

Sample I. yielded a pretty residue consisting of abundant zircon and red-brown rutile, rather rounded and averaging .12 mm. diameter, together with very large kyanite fragments up to .7 mm. long, and coarse staurolite and tourmaline grains. The staurolite is deep golden-brown in colour, of diameter .2 to .3 mm. The tourmaline occurs in grains up to .3 mm. diameter, of brown, greyish, greenish-blue, and deep blue colour. Magnetite, ilmenite, and leucoxene (.2 to .3 mm. diameter) and limonite occur. Small and non-pleochroic dusky grains of andalusite occur less commonly, and chlorite also was seen.

The second sample yielded a rather coarser residue consisting of abundant pleochroic andalusite (.3 mm. diam.), large yellow, grey-brown, and greenish tourmaline (.3 mm. diam.), deep green hornblende (.2 mm. long), and ilmenite. Staurolite and zircon occurred in small grains,

.05 mm. long. Chalcedony was also seen in the sand.

3. BRITISH SANDS.

There does not appear to be anywhere in the British Isles a sand so suitable, from all points of view, as either the Fontainebleau or Lippe sands described above. This does not mean that sands as pure and well-graded as those cannot be found. The question is one also of persistence and constancy to type, that is to say, the bed of sand must be fairly thick, of constant nature and composition, cleanly divided off from other materials, and of considerable extent. In addition, carriage must be cheap and easy, and the deposits situated near the markets.

Undoubtedly there exist in this country large supplies of sand suitable for good flint-glass work, laboratory glass, some table-ware, plate-glass, etc. These deposits certainly yield quite as good supplies as those imported from Holland and Belgium. A full discussion by the writer of our resources will be found in the forthcoming Memoir of the Geological Survey on Glass-sands.

The fetish of "nothing but Fontainebleau" seems now to have disappeared. The notion that only this sand would serve for a great deal of glass-work has been exploded, but in making the best glass, for the reasons given, it still remains unrivalled.

Very few of the numerous British deposits are of sufficiently high quality to be recommended for the making of optical glass.

The following are suggested as being the best, analyses being given in each case.

AYLESBURY SAND.

Worked by Mr. Joseph Arnold (Offices, 32, St. Paul's Road, Camden Town, N.W.).

The quarries occur at Stone, three miles west of Aylesbury. The sand, which is of Lower Greensand age, is also exposed in the "Wind-mill Pit" but is not worked to any extent.

White seams of very pure sand extend to a depth of about 18 feet, but working is then stopped by water. Peaty and ferruginous bands occur, and since the seams of white sand are not very thick (4 to 6 feet) or persistent, working is rather difficult. It is not easy to ensure that successive consignments conform to the highest standard of purity met with. The whiteness and purity are probably connected with the peaty bands. The colour is good, the best Aylesbury sand being better than Belgian, selected samples being practically equal to Fontainebleau sand. Washing does not improve the colour of the best sand to any extent, but a second quality sand washed free from limonitic and clayey pellets in rotary washing plant is also supplied. The latter sand is pale grey but reddens on burning, whereas the former shows little or no change. The chemical composition of a selected sample is as follows:—

SiO ₂	...	99.80	per cent.
Al ₂ O ₃	...	.32	"
Fe ₂ O ₃	...	.03	"
Loss on Ignition	...	.22	"

100.37 per cent.

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The evenness of grade is a marked feature, the sand in this respect very closely resembling Fontainebleau. The mechanical composition is as follows:

> $\frac{1}{8}$ and < 1 mm.	> $\frac{1}{4}$ and < $\frac{1}{2}$	> $\frac{1}{16}$ and < $\frac{1}{4}$
A few grains only	78.3%	15.0%
> $\frac{1}{16}$ and < $\frac{1}{8}$	< $\frac{1}{16}$ mm.	Total sand grade > $\frac{1}{16}$ and < 1 mm.
5.8%	0.9%	93.3%

The detrital mineral suite is the very marked and characteristic one of the Lower Greensand throughout England. Heavy minerals are fairly abundant (.2 to .3 mm. diam.) and the residue consists almost entirely of coarse angular kyanite, staurolite, and brown and blue tourmaline, together with iron-ores (magnetite, limonite, and altered ilmenite), zircon, rutile, etc.

The sands, after being carted to Hartwell Siding near Aylesbury Station, were put on trucks in pre-war times at 7s. 6d. per ton. At the time of writing the price is 10s. This is rather high, and freight charges increase it to about 15s. or 17s. 6d. by the time it reaches a glass-making district as far distant as the London area or Yorkshire.

SAND FROM GODSTONE, SURREY.

The pit revealing the whitest sand, again of Lower Greensand age (Folkestone Beds), is found half a mile north-west of Godstone Church. The whitest and best glass-sand occurs in irregular patches in more ironstained beds. No great supply of standard material can, therefore, be guaranteed, an unfortunate fact in view of its proximity to London and the Thames. It is also some distance from a railway station. Sand has been sold for glass-making, but never in quantities of more than 90 tons a week, an amount hopelessly too small. It is put on rail at Caterham Station at 6s. 6d. a ton. The bulk of the deposit is worked for building purposes.

The sand suitable for glass-making possesses a white to faint yellow colour, at its best almost equal to Fontainebleau sand. It changes to faint pink on burning. Washing would probably improve slightly some of the second-best sand, which would then serve for white-bottle work. The chemical analysis yields:—

SiO ₂	...	...	99.56 per cent.
Al ₂ O ₃	...	...	.26 "
Fe ₂ O ₃	...	...	.06 "
Loss on Ignition	...	...	.24 "
			100.12 per cent.

H. F. HARWOOD, An.

From the point of view of glass-making, the grade analysis is good, being as follows:—

$> \frac{1}{16} \text{ \& } < 1 \text{ mm.}$	$> \frac{1}{16} \text{ \& } < \frac{1}{8}$	$> \frac{1}{16} \text{ \& } < \frac{1}{4}$
0.45%	74.9%	22.6%
$> \frac{1}{16} \text{ \& } < \frac{1}{10}$	$< \frac{1}{16} \text{ mm.}$	Total sand-grade $> \frac{1}{16} \text{ \& } < 1 \text{ mm.}$
1.05%	1.00%	97.95%

In mineral composition, the sand resembles other deposits of the same age, such as those from Aylesbury, etc., described. The list of minerals and descriptions there given holds for the Godstone deposits.

MUCKISH MOUNTAIN SAND, CO. DONEGAL.

The deposit is worked by Sir Pieter Stewart Bam, Ards House, Dunfanaghy. The sand lies on the top and upper slopes of a hill (Muckish Mountain) of quartzite about four miles from the sea and 33 miles N.N.E. of Donegal. The quartzite is of pre-Cambrian age. The sand can be worked very cheaply and brought down to rail or boat by gravity, so that it is hoped that it may be placed on the British market at as low a figure as French and Belgian sands. The purity is high, and the colour a good white. The chemical analysis is as follows:—

SiO ₂	...	...	99.37 per cent.
Al ₂ O ₃	...	...	.36 "
Fe ₂ O ₃	...	...	.05 "
Loss on Ignition	...	...	.12 "
			99.90 per cent.

H. F. HARWOOD, An.

No change occurs on heating except in the occasional brown patches which are met with. The latter, although pale-coloured, are objectionable.

The mechanical analysis is as follows:—

> 1 mm.	> $\frac{1}{4}$ & < 1 mm.	> $\frac{1}{8}$ & < $\frac{1}{4}$	> $\frac{1}{16}$ & < $\frac{1}{8}$
1.43%	9.47%	71.3%	17.0%
> $\frac{1}{16}$ & < $\frac{1}{8}$		< $\frac{1}{16}$	Total sand-grade > $\frac{1}{16}$ & < 1 mm.
0.3%		0.5%	99.2%

and indicates that it errs on the coarse side, but as most of the larger grains are very loosely cemented compound ones, the fact does not militate altogether against the value of the deposit. The sand has been cleared of finer material by agencies of denudation, although it still rests *in situ* on the quartzite. It contains a very small proportion of heavy detrital minerals, little else but zircon being present. The sand was used by the Irish Department of Agriculture in the reproduction of Waterford Glass for the Cork Exhibition, 1902, and good glass has also been made from it in the London area.

OTHER BRITISH DEPOSITS.

Tables of chemical and mechanical analyses are added, and also notes upon some other British sands which do not reach the standard of the above. These are mentioned in case it should become possible to treat them profitably.

The white sands of the Isle of Wight, which were formerly worked for glass-making, occur chiefly in the Headon Hill sands and Lower Bagshot beds of Alum Bay and the adjoining area. They suffer from the objections that they contain yellowish ferruginous patches here and there, are not well-graded, and are somewhat inaccessible and awkward to work.

The Lower Greensand of Leighton Buzzard is on the whole rather coarser than that of Aylesbury, but it is possible to select and work beds of finer material. The colour is not so good as that of the latter sand, but the analysis shows greater purity than the colour leads one to expect.

A decomposed sandstone or quartzite from Bethesda, North Wales, has been put upon the market. In parts it yields a very pure sand, but the joint planes which frequently occur are iron-stained, and the deposit is not well-graded. Before being used it would require both washing and screening (see later).

A very pure Coal Measure Sandstone from Guiseley, near Leeds, has been crushed for use as a glass-sand. Whilst being of excellent whiteness and purity, it suffers from the disadvantages of all crushed rocks, and contains material of varying grades, including some amount of dust.

For details of other British supplies of sands suitable for glass of lower quality, the reader is referred to the coming Memoir of the Geological Survey upon Glass-sands.

SPECIAL GEOLOGICAL CONDITIONS.

With a view to realising the geological conditions under which glass-sands occur, and knowing where to look for suitable supplies, it is instructive to draw attention to several salient facts.

The best-graded sands in the British Isles are the dune and shore sands, but these are never pure enough for good glass work. They are used, for example, in the Dublin bottle industry.

Every example of the whitest and best glass sands indicates that the deposit is associated with carbonaceous (planty) matter. Lippe sand occurs with rafts of braunkohle, Hohenbocka sand is associated with carbonaceous layers, Fontainebleau sand with lignites, Aylesbury and Leighton sand with peaty layers. Some of our purest sandstones occur in the coal measures, and, of second-rate sands, the Headon Hill and Bagshot sands occur with lignites, the Northampton sands (Inferior Oolite) carry plant remains, and the well-known Brora coal of north-east Scotland is associated with white sandstones.

The bleaching of red and yellow sands for a few feet in depth on our heaths is another example of this action. The explanation appears to lie in the reducing action of the planty matter. Ferric compounds are reduced to the ferrous state, and are often carried off in solution by percolating water. Sometimes, however, they remain, and are revealed by the return of the red colour on burning the sand.

Estuarine or lagoon conditions favour the formation of white sands and sandstones. In our search for glass-sands, particularly in the Colonies, we have therefore a valuable indication of the kind of strata in which to look, and the conditions under which we may expect to find the required material. Simplicity of composition and perfection of grading are more likely to be found in deposits of late geological age, as exemplified by the occurrence of the glass-sands of western Europe. It is very improbable that any new and large British supplies of first-class sands will be revealed, but deposits of small extent, at present unknown, probably occur in many cases in proximity to, or along the outcrops of, supplies previously worked.

V.—Treatment of Sands.

A.—WASHING.

Many British sands may be improved by violent agitation with water. The improvement may be threefold; a better grading may result

from the washing off of fine sand and silt grades, the iron percentage may be reduced by the washing off of some of the limonitic coating of the quartz grains or the breaking up of ferruginous pellets, and clayey or dusty matter may be removed.

One outstanding fact, however, remains. It does not appear possible by washing to convert a second or third quality sand into a first quality one. An irreducible minimum of iron-oxide always remains and cannot be washed away.

The washing is usually carried out in this country by means of rotary plant. The sand is fed in at one end of an iron cylinder which is rotated. Water pipes run through the upper part of the cylinder and spray the sand, which is forced down the chamber by a screw-action. The dirty water runs off at the opposite end. The sand is drained and usually is left to dry in the open air. In some cases it is washed twice over.

Although the best sands, such as those from Aylesbury and Fontainebleau, are not improved in the matter of iron-percentage by washing, they are frequently and advisedly treated in this way at the works when they are required for optical and special glass-work. The washing then removes dirt, dust, coal, other organic matter, etc., picked up during transport.

By experimental work with elutriators, etc., washing may be controlled so as to eliminate only the grades which it is desired shall be absent. For example, a current of velocity $3\frac{1}{2}$ mm. per sec. (42 ft. per hour) will carry off all particles of diameter less than .075 mm.

The objection to washing (and the subsequent drying) is the extra expense incurred, this amounting at times, owing to the additional handling and moving necessitated, to 2s. 6d. per ton. Glass-manufacturers naturally prefer washing to be done at the pits, and a clean sand delivered. In this case effective drying must also be carried out at the pit, for carriage cannot be paid upon water.

B. DRYING AND BURNING.

Thorough drying is rarely attempted by exploiters of sands in this country, so has often to be carried out at the glass works. The drying apparatus is usually a form of large oven, but in the United States, tier-dryers, rotary methods, and steam-coils are used.

Apart from the mere improvement in colour as the moisture is driven off, the burning of sand often affords a valuable indication of the approximate amount of iron present. The sands from Lippe, Fontainebleau, Aylesbury, Godstone, and Muckish Mountain among others, show no change, but those from the Isle of Wight, Malton (Yorks.), Lynn, Belgium, Holland, and others, show a slight darkening in grey or brown tints, or a reddening on heating. The change of colour is due to the dehydration of limonite, and consequent formation of iron oxide (Fe_2O_3). The burning registers only the iron present as staining, and not that in the form of iron-bearing minerals, which usually remain unchanged. Black specks of coaly matter, which might otherwise at first sight be confused with magnetite or titanoferrite, then burn out and indicate their organic origin.

C. MAGNETIC METHODS.

Sands bearing a considerable amount of heavy mineral residue are, as previously noted, generally unsuitable. Many of the heavy grains, particularly the oxides and ferro-magnesian silicates, react with the electro-magnet, and may be separated thereby from the quartz. In the United States glass-sands have been freed in this way from magnetite, but it is very doubtful whether such a process would pay in this country.

No chemical methods yet suggested have been both completely effective in cleaning a sand of iron and reasonably cheap at the same time.

D. TREATMENT OF CRUSHED ROCKS.

At the outset it may be pointed out that crushed sandstones and quartzites, or even

crushed forms of silica, such as quartz, have never found much favour with British manufacturers. Similar materials, after suitable treatment, appear to be extensively utilised in the United States. The rock known as the St. Peter's Sandstone, is largely used in this way in the great glass-industry of the Mississippi Basin, but it appears to have been naturally decomposed to some extent, like the Muckish Mountain rock, before it is quarried.

The importance of using well-graded sands especially for optical glass has been emphasized. The crushing of rocks always produces material of all sizes from a certain limit downwards. Mining engineers are still endeavouring to devise a machine that will grind or crush rocks to a fine, even grade without producing dust, which is waste material. The great evil of rock-crushing lies in the quantity of dust produced.

The following table gives a few mechanical analyses of crushed rocks which may be compared with those of Fontainebleau sand. Chemically the rocks are sufficiently pure to make good glass.

Locality.	> 1 mm.	> $\frac{1}{2}$ and < 1 mm.	> $\frac{1}{4}$ and < $\frac{1}{2}$
Port-a-cloy (Irish Silica Co.)	—	3.6%	44.0%
Guiseloy*	—	0.1	40.1
Bothanda	2.6	8.6	77.65
Muckish Mtn.	1.43	9.47	71.3

> $\frac{1}{16}$ and < $\frac{1}{2}$	> $\frac{1}{16}$ and < $\frac{1}{8}$	< $\frac{1}{16}$ mm.	Total sand-grade. > $\frac{1}{16}$ and < 1 mm.
30.8%	9.0%	12.6%	78.4%
44.3	9.2	6.3	84.5
8.85	2.3	—	97.7
17.0	0.3	0.5	99.2

* Sifted to 40 mesh.

To produce an effective sand from this crushed material, the fine grades and dust must be washed away with suitably adjusted water currents, the velocities of which have been determined as a result of elutriation experiments. The mixture must also be sifted to remove coarse grades. The cost of quarrying, crushing, and sifting, has generally been already too great to permit of washing also, the price of the unwashed material being prohibitively high even in the neighbourhood where it is produced. Considering also the high freights, it will be seen that it is improbable on these grounds alone, that crushed rocks even with high silica and low iron content, will replace glass-sands to any extent in this country.

The objections to the use of crushed rock-material in the actual manufacture of glass are serious. It has been pointed out that coarse grains produce "stones," and fine dust "blows out," entangles air bubbles, and gives glass which is not of the same density throughout. It is essential for optical glass more than any other that homogeneity should be obtained and defects and bubbles eliminated. As in the case of crushed quartz-crystals themselves, it is also objected by many glass-manufacturers that the metal produced from the crushed material tends to be "cordy" and "wavy," and to remain "sticky" too long during cooling, which also is protracted more than is desirable. Whether this is the result of the mixture of fine grades, of the presence of two allotropic modifications of quartz (for example, α and β forms differing crystallographically and physically) one possibly as matrix, of the different hydration of the silica acting as cement, or of the presence of inclusions in the quartz, is not known. Investigation is now being carried out upon the question. It is noteworthy that the sand obtained from Muckish Mountain, Co. Donegal, Ireland, is a naturally disintegrated pure pre-Cambrian quartzite. It is often difficult to draw the line

between a true sand and such a disintegrated sandstone or quartzite. The mechanical analysis (see table) indicates, however, that it has been fairly well graded, probably a result of percolating waters for a long period, helped by the fact that the original sandstone was also well graded, and most of the cement has since been removed. It contains rather too much coarse material, but this is very friable, and can be rubbed down in the fingers, compound grains thus breaking up. The analysis should be compared with those of Bethesda material, of which the coarse grades will not break up easily, Guiseley, which was supplied sifted to 40 mesh, Irish Silica Co.'s material, etc. Even with washing and sifting the residual rock-particles are distributed over several grades, and the mixture from a crushed rock is still objectionable.

VI.—General Conclusions and Economic Questions.

The literature of glass-making is not large, and that upon glass-sands is almost non-existent. Systematic surveys of the sand resources of countries do not appear yet to have been published, and the returns dealing with glass-sands are very few.¹

Eminently suitable sands of a high degree of purity and in abundant quantities have before the War been imported into this country at a comparatively cheap rate. Most of the supplies came from Holland, Belgium, and France. Indeed, the only sand used for optical glass and table ware was brought from Fontainebleau, near Paris. It should be emphasised that there appears to be no sand in the British Isles equal

¹ The Geological Surveys of the United States and of Denmark have published small notices.

U.S.G.S. Bulletins 285 and 315, and Annual Reports of Mineral Resources 1883 onwards.

Danmarks Geol. Undersøg, Ser. 2, No. 16. 1905.

in quality or extent to that from Fontainebleau. Sands have been mentioned in a previous section which are equal in chemical purity and grade composition, but their extent is comparatively small, and successive consignments cannot be guaranteed absolutely true to the best sample. Variability in such an essential raw material is abhorred by glass-makers. Since these foreign supplies were brought by water almost from their home into the estuaries and canals which give access to our industrial areas (often as ballast on the return journey of vessels carrying coal, etc., out) sands were abundant and cheap, far cheaper than railway-carried British material. The result was thoroughly to spoil glass manufacturers, and they smilingly admit it. Consignment after consignment was so true to sample that apparently no need existed for a works-chemist and laboratory. The reasons for the suitability of the sand were not determined. Manufacturers were able to go forward with very little alteration of the batch, and without any apparent necessity for investigation and analysis. British supplies, had they been as pure, extensive, and available, would not have been used on account of their greater cost.

Owing to the War and the consequent dislocation of traffic and shortage of labour and ships, the foreign supplies have been much restricted, and in many cases stopped. In view of the difficulties thus experienced by the glass trade, it behoves us to see whether we must be dependent hereafter upon the foreign supplies, or whether we possess reserves of suitable sands at home or in the Colonies. We cannot ignore the future; foreign supplies, in particular those of Fontainebleau, are not unlimited, nor will they always arrive in this country so cheaply or of so uniform a quality. The output may at any time be restricted for domestic reasons, or a tariff imposed.

The margin of profit on sand is small. Where

a material is used in such large quantities as sand, it must be cheap, and therefore abundant, unconsolidated, easily and cheaply worked, and accessible and conveniently situated. Seams or beds must run very true in composition, and be clearly marked off from other beds or materials. Due consideration must be given to the important question of workability. The factors to be considered in recommending sources of sands for any industry should include the quantity available, the location with respect to fuel supplies and markets, and to transporting routes, road, rail, canal, or sea. The accessibility of deposits in sea-cliffs, on hills, near bogs or marshes, as well as the conditions of quarrying, such as workable depth below ground-level, position of the water-table, thickness of overburden, etc., must be considered. The state of the local labour market also cannot be ignored.

Carriage by road is expensive, and railway freights are even more so. Foreign sands could actually be bought in this country at a price per ton lower than the cost per ton of moving British sands a comparatively short distance. The freights frequently amount to five or six times the cost of the sand at the pit. Canal-traffic must replace rail-transport (unless the tariff of the latter is reduced), if English sand resources are to be developed. Reorganisation and revival of canal-transport must, therefore, take place.

While the imposition of greater import duties upon foreign sands may not be desirable, it may have to be considered. In any case we shall do well to enquire into our Colonial resources of sand, particularly for moulding purposes as well as glass-making. For very special glasses such as optical glass, the variation in cost of sand is small compared with the considerable cost of production. It may be advisable to be independent of foreign supplies and, if suitable sands do not exist in this country, to fetch them in from distant Colonies, bearing in mind the

TABLE OF CHEMICAL ANALYSES OF GLASS-SANDS.

	SiO ₂	Al ₂ O ₃	Fe ₂ O ₃	CaO	Loss on Ignition.	Age.
Lippe I.	99.88	.18	.02	n.d.*	.21	Miocene
Lippe II.	99.73	.20	.08	n.d.	.23	"
Fontainebleau	99.65	.21	.04	n.d.	.16	Oligocene
Belgium	99.88	.30	.03	n.d.	.23	?
Aylesbury, (Arnold's)	99.80	.32	.03	n.d.	.22	Lr. Greensand
Godstone	99.56	.26	.06	n.d.	.24	"
Leighton, (Arnold's) ..	99.69	.25	.21	n.d.	.27	"
+Lynn, white ..	98.47	.81	.24	.26	.22	"
+ " washed ..	98.54	.61	.26	.22	.37	"
+South Cave, Yorks....	97.64	.181	.089	trace	1.980 & Water	Calloviau.
					.072	
Bethesda, N. Wales ...	99.85	.54	.04	n.d.	.86	?
+Guiseley, Yorks.	98.5	1.25	trace	.25	n.d.	Coal Measures
Muckish Mountain ...	99.87	.36	.05	n.d.	.12	Dalradian

* Where Lime was not determined, only a trace, if any, was present.

† Analyses not carried out in the Imperial College.

MECHANICAL ANALYSES OF GLASS-SANDS.—Continued.
Diameter of Grains in mms.

	< $\frac{1}{16}$ mm.	> $\frac{1}{16}$ and < $\frac{1}{8}$	> $\frac{1}{8}$ and < $\frac{1}{4}$	> $\frac{1}{4}$ and < $\frac{1}{2}$	> $\frac{1}{2}$ and < $\frac{3}{4}$	< $\frac{1}{16}$	Total Sand Grade > $\frac{1}{16}$ mm.
Corby, (Northampton Sands) ...	—	1.3	78.5	16.0	9.3	74.8	
Charlton, Kent (Thanet Beds) ...	—	16.2	79.6	8.1	1.1	95.8	
Hoathley, (Tunbridge Wells Sands) ...	—	28.8	69.7	5.8	0.7	93.5	
Levensat, N.B. (Millstone Grit) ...	8.7	88.5	6.0	0.2	1.6	98.2	
Workop, (Trias) ...	> 1mm. 2.0 > $\frac{1}{4}$ mm. 6.6	58.65	26.15	4.5	2.1	98.4	
Silver Strand, Wicklow (blown sand)	—	92.1	6.4	0.9	0.6	98.5	
Bamburgh, (blown sand) ...	—	94.8	4.7	0.5	—	99.5	
Spital, Cheshire (Trias) ...	2.2	75.9	17.05	4.85	—	95.15	
Muckish Mountain: (pre-Cambrian) ...	> 1mm. 1.48 > $\frac{1}{4}$ mm. 9.47	71.3	17.0	0.8	0.5	99.2	
Bethesda ...	> 1mm. 2.6 > $\frac{1}{4}$ mm. 8.6	77.65	8.85	2.3	—	97.7	
Guiseley (Coal Measures) ...	0.1	40.1	44.3	9.2	6.8	84.5	
Port-a-cloy ...	8.6	44.0	30.8	9.0	12.6	78.4	
Westport (crushed silica) ...	—	2.6	40.6	88.8	18.0	48.2	

principle that the Empire should be self-supporting and the union in industrial and scientific matters between the Mother-country and the Colonies as close as possible.

In the course of the investigation into British resources of sand, with particular reference to glass-making; I have received every courtesy from glass-manufacturers and works-managers, as well as from owners and exploiters of sand-pits and quarries. I therefore express my indebtedness to them, and desire also to record my thanks to Dr. Walter Rosenhain, of the National Physical Laboratory, for discussing many questions with me, and to Dr. H. F. Harwood of the Chemical Department, Imperial College of Science and Technology, who has kindly carried out many of the chemical analyses which form so valuable a contribution to the paper.

Discussion.

DR. ROSENHAIN said it was not often that one had the pleasure of hearing so scientific and yet so practical a treatment of such a subject as that—a subject which to many would appear on the face of it singularly dull and uninteresting. With regard to the historical aspect of the matter, he thought Dr. Boswell had been a little hard on the glass manufacturer, and had ignored the fact that after all the geologists also might have been somewhat to blame for the neglect of the glass sands of this country. Twelve or fifteen years ago he had been engaged on the practical aspect of the question; and when he had sought the help of geologists at the Museum in Jermyn Street and elsewhere they had told him practically nothing, and he had had to spend his own time in going to the various sand beds in the country to find out what our resources were. He was bound to say that the results of his own investigations had been very similar to those arrived at by Dr. Boswell; it was extremely difficult to find in this country any satisfactory substitutes for the

Fontainebleau and other foreign sands. He thoroughly endorsed Dr. Boswell's view that the resources of the outer Empire should be tapped for this purpose, and emphasised his remark that the cost of sand for optical glass making was nothing like so important a factor as in ordinary commercial glass making, even of the better kinds, for other than optical purposes. The great items which went to make up the cost of manufacturing optical glass were fuel, labour, and plant, and, above all, the very small amount of usable glass which was obtained from each smelting. The actual cost of the glass itself was not a vital factor.

From the purely optical point of view, one thing which Dr. Boswell had said was open to criticism, namely, as to grading. The importance of grading was fully recognised by manufacturers of optical glass. While uneven grading undoubtedly tended to create the difficulties Dr. Boswell had referred to, he was not at all certain that in optical glass making that was a serious matter, because the duration of the melting was much longer than in other glass making, and for other reasons, quite apart from the analysis of the glass, stirring was absolutely essential; so that he doubted whether any little inequalities due to unsatisfactory grading would really add much to the difficulty of producing homogeneous glass. He would particularly suggest one point as of importance, namely, that the use of extremely fine grades of material, the use and the introduction of silica in extremely fine forms, was desirable from another point of view, namely, that of rapidity of chemical action, and consequently of rapidity of melting. So much was this the case that he believed certain conscientious manufacturers had begun to grind the whole of their glass materials down in expensive appliances in order to get it down to those finer grades which Dr. Boswell rather condemned in glass sands. Not only that, but the use of precipitated silica had

been suggested, and he was not at all certain that there was not a good deal of foundation for that suggestion from the point of view of furnace economy. Of course, it was necessary to get a certain degree of uniformity, or there would be a waste of the time gained in the rapid melting of the fine materials in waiting for the coarser ones to melt. The question of the possibility of using precipitated silica was undoubtedly an important one, and provided the material could be obtained in sufficient quantity and of sufficient purity, it might solve the whole difficulty so far as optical glass was concerned. He did not think the quantities procurable were sufficient for plate or sheet glass, but for optical purposes it might be extremely useful.

Dr. Boswell had referred to the analyses of sands, and he thought no one would be inclined to quarrel with his strictures on the kind of analyses one met with up and down the country. He did not himself quite understand the analyses Dr. Boswell had shown them on the screen, but as they were not his own it would perhaps be hardly fair to cross-examine him about them. Still, his own impression certainly was that the Lippe sands ran to over 99.5% SiO_2 . The analysis was not quite clear, because there was loss on ignition, and the whole added up to considerably over 100. None the less, the order of the sands dealt with by Dr. Boswell—Lippe I., Lippe II., Fontainebleau, Belgian—agreed with his own observations, so that the variations were probably a matter of presentation only, and did not indicate any real difference. The question of drying was another important matter. One of the great objects of optical glass making was uniformity of composition. If one introduced, say, 60 per cent. of silica, and that varied by one or two per cent., a result was obtained which undoubtedly affected the optical properties of the glass in the fourth decimal place. So that the degree of

drying must either be kept extremely perfect or constant, or the moisture must be estimated carefully every time a batch was made. He was afraid that was almost the only way, because sand varied in moisture even if thoroughly dried. With these few remarks he would leave the subject, although there was a great deal more which could be said. The matter was one of great importance, and he welcomed this, the very first opportunity the Society had had of ventilating the subject on something like a sound scientific basis. (Applause.)

Washing Sand, Emery, etc.

DR. CLAY said that some time ago—he thought just about the time Mr. Crook was making his experiments—he also had made some experiments on the possibility of grading substances like sand by the method of causing a constant flow of water to pass through them, and he had found a difficulty, which no doubt had been experienced by others working in the same field. Owing to viscosity, the water at the middle of the tube travelled with a much higher velocity than the water near the walls of the tube, with the result that the size of the particles falling through a uniform tube with a uniform flow of water was not uniform; the particles falling down the centre of the tube were much larger than those coming down the sides of the tube where the water was travelling more slowly. Possibly by some stirring arrangement, and by repeating the experiment over and over again, or carrying it on, as he understood Dr. Boswell had done, for a long time, the small particles would ultimately be carried up the middle, and only the larger particles would ultimately remain. But if the apparatus was made on a much larger scale, by using the central part of a tube of considerable size, where the viscosity effect would be negligible, a uniform flow of water might be obtained

up through this central part of the tube, and the particles falling through this would be uniform in size. It might thus be an economy to use a larger tube and a bigger volume of water for a shorter length of time. It had occurred to him that the same method of separating substances by a flow of water might be utilised for separating the glass waste obtained in lens grinding and polishing, as ordinarily there was a large waste of emery and carborundum in glass grinding. Of course, the difficulty lay in the fact that the emery particles were not all of one size, and therefore the smaller emery particles would be carried away with the stream which left the larger glass particles behind. To succeed it might be necessary to obtain uniformity of size, first, by means of a sieve, after which the separation of the glass and emery could be effected by this apparatus.

MR. CHALMERS thought the question of the materials to be used in making optical glass was very important indeed, and that some of the methods Dr. Boswell had dealt with would perhaps lead to good results in other branches of the optical industry.

MR. W. SHACKLETON remarked that, although Dr. Rosenhain had mentioned that the availability of British sands for optical glass-making purposes had already been investigated, he (Mr. Shackleton) had no recollection of the results of those investigations having been made public. Possibly the investigations had been undertaken on behalf of a private firm, and their results were not, therefore, available to anyone making subsequent inquiries. The method of elutriation described by Dr. Boswell was, he thought, extremely interesting, and it might be used in a practical way in optical workshops. He had been that morning in the glass-making district of West Lancashire, and he had learnt that they were there using Lancashire sands. He had not noticed any mention of these Lancashire sands in Dr. Boswell's paper.

DR. BOSWELL asked whether the sands in question were used for making optical glass.

MR. W. SHACKLETON replied in the negative; the sands he referred to were used for making plate glass.

DR. BOSWELL said he was aware of the existence of those sands, but in his paper he had confined himself to the question of optical glass sands.

MR. W. SHACKLETON went on to say that he had already approached Dr. Boswell on the subject of giving them a paper at some future time on the subject of the abrasive properties of different sands, and had received a favourable reply. In the large glass works where plate glass was made, sand, of course, was the first material used for grinding; emery came later. The sands used in the St. Helens district were obtained from the Mersey, and at St. Helens there were great heaps of sand dunes which had been collected for over half-a-century. St. Helens, he might say, presented almost the appearance, on its outskirts, of having been "at the Front," and of having been subjected to a series of mining operations. It was said that when the Zeppelins came over there and saw all those sand-heaps, they said, "Oh, some of the others have been here already," so they did not drop any bombs there, and went away again. (Laughter.)

THE PRESIDENT said the mere mention of sand reminded him of one of the most bitter disappointments of his life. When he was a very small boy he had read a book by a very attractive writer, who solemnly stated that the Lord's Prayer was "written on every grain of sand." Well, he had believed it. (Laughter.) He was at that age when one did believe everything one was told. (Renewed laughter.) And when he later discovered that the microscope failed to reveal the Lord's Prayer, written in English characters—because that was what he had fully expected to find—he must admit that

he was for some little time afterwards more or less an atheistical desperado. (Laughter.)

With regard to the question of the purity of sands, he had some figures which might throw some light on the subject. Some years ago he had been in Germany when some very important work was being done on vitreous compounds generally, not only with regard to glass, but with regard to quartz and other substances, and a friend of his who had done a great deal of the work in question had told him a very remarkable fact in connection with it. They had no difficulty in getting sand with a pure silica content of 99.96, and it was very cheap; but it was nothing like pure enough for the particular experimental research upon which they were engaged; and in order to get rid of that .04 per cent. of impurity it was necessary to run the cost up to something like £10 sterling per 11b. weight. The research upon which they were then engaged was a failure, and the cost was many thousands of pounds; but his friend had assured him that during the course of that research they had succeeded in discovering a method of purifying the sand which more than repaid them for the whole cost of the research, and they had obtained the sand, finally, of the necessary purity at a cost of something like 1s. 3d. per lb.

There was one little matter he might mention which he was afraid had not much to do with sand, except that it indicated a method by which sand might be done away with altogether for glass-making. He had the permission of Mr. Swift, whom he was glad to see present that night as a member of the Society, to say that a short time since he had come across a most extraordinary thing in connection with the construction of microscopic objectives of the very best class. He had a lens to repair, made by a very famous German maker, and to his amazement, in warming up one of the component systems, it almost exploded, and went

into a kind of white enamel. Of course, he was at once anxious to know what was the matter, and he had finally succeeded in getting this lens—which was a deeply curved negative lens, cemented between two ordinary glass lenses—analysed when it was found to consist of potash alum. That would seem to indicate very strongly that, at any rate so far as rare glasses are concerned, glasses for which there was very little demand indeed, there was a possibility of a great deal of good work being done by a thorough research amongst natural and artificial minerals, as suggested by Dr. Rosenhain some years ago. If potash alum, for instance, was capable of taking such an important part as it did in apochromatic objectives, it might be found that there were other natural and artificial minerals which might be applied to the same purposes.

Reply.

DR. BOSWELL, in replying on the discussion, said that he should be happy to talk about the abrasives used in glass manufacture a little later, when there was a little more British material available. With regard to Dr. Rosenhain's opening remarks, he thought he had stated very clearly in his paper that geologists were equally culpable with glass manufacturers in not having investigated the glass-making resources of their own country. Geologists did not want to shirk responsibility in the matter in any way, and he hoped he might be pardoned if he had appeared to be too harsh in his remarks on glass manufacturers. At the same time, the latter had admitted that they had been spoilt by the ease with which foreign sands had been obtained, and the cheapness and constancy of those sands. It must also be remembered that Dr. Rosenhain had had experience of one of the most advanced firms, and was himself one of the few really scientific men who had ever gone into the question. As the Hon. Secre-

tary had pointed out, there was, he thought, no published statement of the result of Dr. Rosenhain's investigations as to whether or not there were deposits of British sands suitable for the manufacture of optical glass; and he doubted whether Dr. Rosenhain's firm had thought fit to publish such information for the benefit of all the other optical glass makers. If they had done so there would have been less reason early in the war for calling upon those whom he represented to resolve the difficulties of many of the manufacturers.

He had expected to be hit much harder than he had been when he had consented to come there as a geologist. Geologists had been requested to go into the question of British sand resources, amongst others, and to do what they could to help industry. They had to begin by studying what the glass manufacturers wanted. He himself had had, at the outset, a hopeless ignorance of methods and materials, and he was only grateful to those manufacturers who had given him many pleasant days in their works, enabling him to see, as far as possible, what it was they wanted. But then he had found himself up against the optical glass problem. Glass manufacturers had been very reticent in the past, especially on the question of optical glass, so that the chances of a mere outsider getting information about the processes, and the reasons for the material required, must be very small. That, therefore, must be his excuse if he had made any bad blunders on the optical side. He had not wished to imply that there had been any neglect at all on the part of business men, but he did not think they could carry out such a thorough investigation into British resources as could those whom he represented, who had naturally had to do with such materials for some considerable time.

He had acknowledged in his paper (but had inadvertently omitted to mention it at the meeting) the kindness of Dr. Rosenhain in letting

him have samples of the Lippe sands for analysis. The point he (Dr. Rosenhain) had noticed was quite correct; the mechanical analysis of the Lippe sand showed 1.1 per cent. of material less than 1/100 mm. in diameter; that was estimated by difference, and he could not, and did not, believe that that was clay grade; there was absolutely no clay there. The fact was he had omitted to dry that sand carefully before it was submitted to either chemical or mechanical analysis, and that was undoubtedly where the discrepancy came in; water there was estimated by difference as clay grade. Water also appeared in the loss on ignition in the chemical analysis. He also had it on Dr. Rosenhain's authority that the Lippe sand was guaranteed 99.8% silica when sold. The silica percentage doubtless varied a little with the samples. In the case of the Fontainebleau sand his own analysis showed 99.7 silica, which was an indication that the sample he took was higher in silica than another sample quoted in the tables. He was quite in agreement with Dr. Rosenhain, therefore, on that point. Of course, he ought to have emphasised that in the case of optical glass, where stirring was actually carried on, the question of grading the sand was not so important; he had been speaking in that connection of glass making generally. In the vast majority of British works there were no arrangements for stirring the glass, and therefore grading was essential. But he might mention that there was one firm which was actually grinding the whole of its batch for making chemical glass-ware. It was a very expensive process, and he doubted whether it was worth while.

Elutriation Processes.

With regard to the question of grading, "the proof of the pudding was in the eating," and the fact was the elutriation apparatus did actually grade the sand fairly accurately, and that the

apparent vortex and eddy action did not necessarily affect the grading seriously was shown by examination of the grains under the microscope: there was no great admixture. With regard to the separation of emery and glass, that would be possible, of course, if the emery was uniformly much finer than the powdered glass, or, on the other hand, if the reverse held. Elutriation could also be adopted for separation if the grains of each were of the same size. If, however, there were mixtures of different kinds of particles of different sizes, then elutriation could not be adopted, because the fine emery, which was heavier, would occur in the same grade as the coarse glass. In that case he would suggest their turning to mining engineers' books. It might be possible to "pan" the emery by centrifugal action, or to try separation by means of an oil—he did not know what affinity emery had for oil—or many other methods might be looked into with a view to effecting the object in question. The question of expense would of course come in, but possibly some of the works on ore dressing might give some indication. At any rate, it was a case for experiment, and if some of the mixed material were submitted to him he would be pleased to examine it and report upon methods of examination. In that connection air separation had been used abroad instead of elutriation by water: by putting on a blast, the fine materials which were apt to choke the pipes of furnaces were carried off. He was assured that that method worked in preparing sands for filtration purposes.

He concluded by thanking the members present for their kind reception of his short talk upon glass sands.

RADIUM AND OTHER LUMINOUS COMPOUNDS.

By MR. F. HARRISON GLEW.

Delivered 8th June, 1916.

THE term Luminescence deals with the production of light at ordinary temperatures as distinguished from Incandescence with the production of light at high temperatures.

Prehistoric man must have been familiar with the remarkable production of light by fireflies and glowworms. It is not to be wondered at that dread and superstition is attached to these objects. In these instances the phosphorescent glow has been shown by Mr. Phillips to be due entirely to the oxidation of an organic fluid secreted by the insect. If this fluid is separated from the insect the glow continues and can be accelerated by a draught of air or by using oxygen. It is stated that the light of the tropical firefly is capable of illuminating a white watch dial at a distance of 10 feet, although the light of the firefly does not appear to have been accurately measured in candle power.* Lord Rayleigh says that when in Ceylon he estimated the light to be equal to that of a second magnitude star, when at a distance of 20 feet or 30 feet from the fly. It is clear that the phenomenon is of a chemical nature, and comes under the term of Chemi-Luminescence. Phosphorus may be included in this class. This is another case of slow oxidation in the air.

An interesting example of Chemi-Luminescence can be exhibited by the rapid oxidation of pyrogallol acid and other organic substances in the liquid form by mixing a solution with

* I find that Langley and Very measured the light from the Cuban Firefly and found it to be $1/1,600$ candle.

peroxide of hydrogen and formalin. This results in production of light with a rich red glow which quickly dies down, leaving a weaker luminosity of the liquid mixture which may persist for half an hour. Research in this direction is much needed, and might lead to important results.

Luminescence resulting from a previous exposure to light is termed Phosphorescence, if the emission is of perceptible duration. In some cases the storage and emission of light is almost instantaneous, and not capable of detection without suitable apparatus. This phenomenon is known as fluorescence. The only distinction between fluorescence and phosphorescence is the relative duration of emission. Expt.

The phenomena of phosphorescence brings us in touch with the border land of physics and chemistry.

Phosphorescent substances exhibit the photo-electric effect, that is they readily part with electrons when illuminated by ultra-violet light.

Phosphorescence of the highest intensity can only be observed a moment after excitation, as there is a rapid fall in luminosity on removal from the source. I now show the effect by the rapid rotation of wooden cylinders which have been painted with the phosphorescent substances; the exciting arc lamp is hidden from the observer by the rotating cylinder, which becomes more luminous with increased rapidity of rotation. The apparatus shown is capable of giving 10,000 rotations per minute; in this way we can easily examine the spectrum and also measure the surface brightness against a 16 candle-power lamp, which, as you see, has to be brought within a few inches of a white card to balance with the direct light of the phosphorescent body, in this case zinc sulphide.

A phosphorescing plate cannot excite another plate of the same substance, as the exciting rays must be of a higher order than those emitted, according to Stokes. Although zinc sulphide

cannot be excited by its own light, it can be made to phosphoresce by the light from calcium sulphide which gives out rays of shorter wave length—for the same reason zinc sulphide cannot excite calcium sulphide; these experiments can be easily shown by the superposition of coated plates, one of which is charged with light.

Luminescence may also be produced by friction by scratching or rubbing certain crystals, such as quartz, sugar, fluorspar, pentadecylparafolyketone, benzyl, beta, naphthylamine. The latter is extremely bright. (Experiments shown.) This production of light by friction is termed Tribo-Luminescence.

Some crystals which have been previously exposed to light or radiant energy emit light on being heated, the required temperature being much below red heat. This phenomenon is termed Pyro-Luminescence. (Experiments shown.) This sample of Spodumene, composed of silicates of lithium and aluminium, has been in the proximity of a large quantity (50 milligrams) of Radium, the energy of which has been stored up in it. On placing the crystal in boiling water or otherwise raising the temperature the whole crystal becomes intensely luminous with a rich orange glow. This is a beautiful instance of the storage of radio-active energy which can be released at will. Moreover, the experiment can be repeated time after time, the emission is under perfect control: it may be temporarily suspended or accelerated. In this wonderful crystal when in this ionised condition, the emission form of visible light can also be obtained by exposure to light of a lower order of refrangibility. The release appears to be nearly directly proportional to the duration of the exposure. This is another instance of Stokes' law. In this particular instance the energy which has been stored is the result of Gamma radiation, which we now know to be of the same nature as ultra-violet light. The phenomenon is therefore analogous to the storage of

violet light in luminous paint, which light can be released by red light. (Experiment shown.)

Luminous paints are in great demand, especially for naval and military purposes. There appears to be some misconceptions as to the various purposes and relative advantages of different kinds of paint, some of which are excited by daylight which they are capable of storing up, slowly emitting the same at night. However, the luminosity falls off very rapidly after the first hour or two after exposure. This class of paint was originally prepared in the days of Alchemy by calcining crystals of sulphate of barium with organic matter, such as flour, thus converting the sulphate into a sulphide. This was originally prepared at Bologna, and is known as Bologna Phosphorus.

Another form of daylight paint was made by roasting oyster shell with sulphur by Canton, and known as Canton's Phosphorus. This paint was very much improved by Balmain, it being found that the addition of very minute quantities of other substances—of which bismuth is one—added very greatly to the luminosity.

SELF-LUMINOUS PAINTS.

The discovery of radium makes it possible to produce luminous compounds which are quite independent of daylight excitation as the necessary energy is being given out continuously by the radium rays, which are absorbed by certain crystals which give out their own characteristic light, such as sulphide of zinc. The luminosity produced depends on the proportion of radium. The luminosity is continuous night and day independent of its surroundings.

Radium is wasting itself away at a rate which will reduce it to half the quantity in two thousand years. Having this fact in view many people suppose that the luminosity will fade away at the same rate, which actually would be the case if the zinc sulphide did not undergo deterioration. As a result of many years' ex-

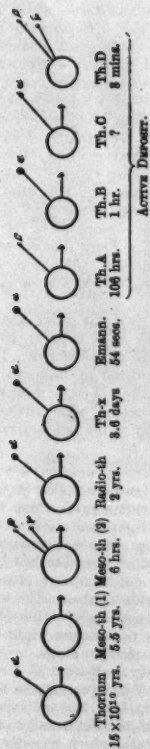


Fig. 1.—Thorium Series (Rutherford).

perience, it has become evident to me that the zinc sulphide undergoes deterioration much more rapidly than is generally suspected. Moreover, the rate of decay in luminosity is in proportion to the initial luminosity. The luminosity is proportional to the amount of radium present—but not exactly proportional.

Radium paint prepared according to Admiralty specification contains 0.4 mgms. of radium bromide, or its equivalent, has a luminosity of about .03 foot candles. Whereas the paint containing half this amount of radium, namely 0.2, is more than half the luminosity.

Decay of Self-Luminous Paints.—The luminosity being 60 and 70 p.c. so far as measurements have been made with a set of samples which have been found to decay in proportion to their respective luminosities. Therefore the .4 sample will die down at a much more rapid rate than the 0.2 sample.

Two such examples prepared at the same time in October last year which have now nearly reached (after eight months) equal luminosity, indicating that the weaker preparation will have a longer life. These facts do not appear to be generally known.

Makers of radium paints appear to think that paints containing the most radium are in consequence the more luminous, and will last longer than weaker preparations. Whereas the facts are the exact reverse.

Other Radio Active Paints.—For the purpose of making the self-luminous paints other substances than radium can be used sometimes with advantage, depending on considerations of cost and durability.

Radio-active substances give out three distinct kinds of radiation (Alpha, Beta and Gamma). It is important to consider which kind of ray is most effective in the production of light by a bombardment of the crystals of sulphide of zinc.

Experiments show (experiments shown) that the alpha radiation is by far the most effective.

Each Alpha particle consists of one atom of helium carrying a positive electric charge.

In the case of radium, this particle is shot off with the initial velocity of about twelve thousand miles per second. In its course, passing through matter, it shatters ninety thousand molecules of the substance (solid, liquid or gaseous). The whole time occupied in doing this does not exceed $1/1,000$ of a millionth of a sec-



FIG. 2.—GLASS TUBE COATED INTERNALLY WITH SULPHIDE OF ZINC.—BRIGHT PATCH IN CENTRE PRODUCED BY LOCAL COOLING SHOWING THAT THE RADIUM EMANATION PRODUCTS CAN BE CONDENSED.

ond. In this short space of time the alpha particle of radium will travel through about 7 centimeters of air at ordinary pressure, leaving a track of ninety thousand ions which Wilson has made visible by sudden condensation of aqueous vapour on them. (Slide shown.) The length of path in other substances is simply proportional to the density.

Let us consider the path of the alpha particle in the zinc sulphide. The length of the path will be much shorter than in air, but it will pass through several crystals in succession, creating in them the total of about ninety thousand ions. It is with these ions that we are concerned in the production of light. Each ion forms a centre for a re-adjustment of the chemical affinities of the adjacent molecules. Electrons having been knocked out of some atoms, have become attached

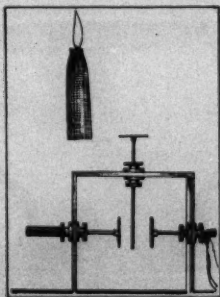


FIG. 3. — THORIUM MANTLE DISCHARGING ELECTROSCOPE.

to adjacent molecules. This means storing up energy originally derived from the alpha particle. It would appear that energy stored in this form produces new compounds which are not stable, and that the ultimate re-arrangement of the electrons is attended by the emission of light. The alpha particle from thorium "D" has a longer range (8.6 cms.) than that of radium (7 cms.). It consequently carries more energy and covers a longer path in any substance. It is consequently more effective in producing luminosity. Against this advantage in the use of thorium products we have the dis-

advantage of the relatively short life compared with radium.

Mesothorium decays to half activity in 5.5 years without giving off alpha rays. It disintegrates into radio-thorium and other products which do give out alpha rays, and is more effective than radium, weight for weight. The cost of mesothorium is not very much less than that of radium, against which it is measured on its Gamma radiation.

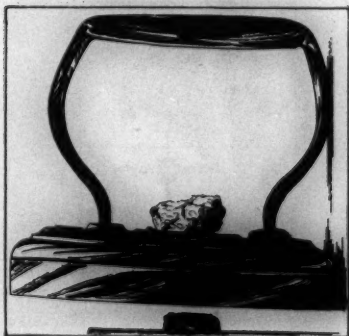


FIG. 4.—RADIUM MAKING WILLEMITTE LUMINOUS THROUGH A LAUNDRY IRON.

Now the Beta and Gamma radiation of pure mesothorium is about one hundred times greater than that from the radium C., so that much less than a molecular weight has to be paid for at a much higher price than radium. This point is often missed when a theoretical efficiency is worked out under the relative molecular weights of mesothorium and radium. When all these facts are taken into consideration, the relative cost of luminous paint made with radium or

mesothorium is not so largely in favour of the latter as would appear from theoretical calculations based on molecular weights. On the whole it would be advantageous to use mesothorium if it had a life equal to that of radium, the half period of which is two thousand years against 5.5 for mesothorium, of which one quarter only would remain at the end of eleven years. In fact, loss in money value of the radium in a life time is negligible, whereas with mesothorium the loss is complete, except for a residue of radium of about 20 per cent., which is always



FIG. 5.—AUTO-RADIOGRAPH OF OLD THORIUM MANTLE.

present in mesothorium, which has been extracted from monazite. This radium is chemically inseparable.

So far as I know all mesothorium on the market is of enemy origin, a consideration which is of paramount importance at the present time. The further advantage of the use of radium is that it can be readily recovered from luminous compounds which have lost their luminosity.

This can be made up again with fresh zinc sulphide from time to time as may be necessary.

Some time ago I was puzzled at finding that some samples of radium were better than others for manufacturing luminous paint, the explanation is that old radium is richer in the disintegration product radium "F"; in fact, the Alpha radiation of radium increases for the first 100 years.

Ionium suggests itself as a substitute for radium. At present it is not obtainable in a state of purity, or it would be an ideal excitant. The radiation consisting of alpha particles only, the half period is even longer than that of radium. I would strongly advise manufacturers to try to save and concentrate this product; also the actinium, which also might be made use of for this and other purposes. All these, including the radium F (polonium) are rarely extracted by manufacturers.

The nation would be well repaid if the Government subsidised the necessary research work.

Measurement of Luminosity.—The spectroscopic examination of luminous zinc sulphide shows that the light is almost entirely confined to the yellow green. It is fortunately possible to use a green screen of dyed gelatine, which transmits light in this region only, excluding blue and red. By using such a screen it is possible to measure the luminosity of the paint compound against a standard candle of white light. If the standard light is reflected from a white surface of cardboard, it is possible to compare this surface with the direct light from the luminous compound which should be shielded from the light of the lamp. The observation being made through the coloured gelatine, or as an alternative, the colour screen may be interposed between the lamp and the reflecting surface, the distance of the lamp being variable in the usual way. The sample of luminous compound to be tested must have been sealed up one month before testing.

For strict accuracy in photometric tests due allowance for coloration of the glass should be made.

The coloration of glass can be reduced, and in many cases entirely removed by exposure to sunlight for a few days. This procedure does not endanger the zinc sulphide.

I now show a sample, part of which has been exposed to daylight and sunlight throughout two years. There is no apparent difference between the exposed and the protected control portion.

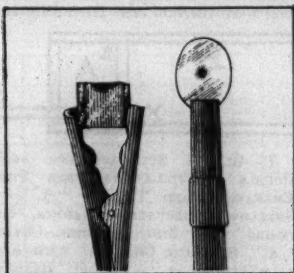


FIG. 6.—CUBE OF GLASS COLOURED BY GAMMA RAYS, ALSO A QUARTZ LENS IONISED BY SAME.

so there is no disadvantage in exposing painted surfaces to daylight, in fact I consider it an advantage.

Measurement of Radio Activity and Purity.—

The radium content of luminous paint can be made by using the Gamma Electroscope in the usual way, making due allowance for the self absorption of the zinc sulphide and the thickness of the glass in which the compound is contained, which need not be opened. For the

detection of mesothorium it is necessary to expose the compound itself or the emanation in an emanation electroscope which is capable of revealing the presence and relative proportion of radium and thorium compounds.

The presence of mesothorium may also be detected by observing the scintillation produced by the Alpha particles in a specially made scintilloscope (shown), which distinguishes between radium and thorium emanation. The scintillations from thorium occur in pairs, due to the fact that when thorium emanation disintegrates

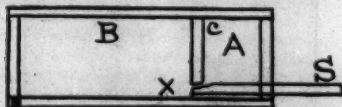


FIG. 7.—GLEW'S SCINTILLOSCOPE SHOWING DOUBLE SCINTILLATIONS FROM THORIUM EMANATION, AND THORIUM "A."—"B," SHALLOW OBSERVATION CHAMBER, COATED INSIDE WITH ZINC SULPHIDE CRYSTALS. "A," SEPARATE CHAMBER, WITH ADJUSTABLE OPENING AT "X." "S," REGULATING STOPPER. THE MESO-THORIUM IS PLACED AT "C." THE EMANATION SLOWLY DIFFUSES THROUGH "X" INTO "B" CHAMBER, WHERE DOUBLE SCINTILLATIONS SHOW UNDER MICROSCOPE.

it gives out an Alpha particle by reason of which it becomes transformed into thorium "A," which in its turn disintegrates in the fifth of a second giving out another Alpha particle. Thus two scintillations are seen practically as a double, the interval of time, one fifth of a second, being only just appreciable. When double scintillations of this character are seen it is certain that thorium compounds are present.

Uses for Luminous Compounds.—Obviously daylight luminous paints can only be used in

exposed situations, so that daylight can be freely absorbed to be again given out at night. These are useful in shipping and a great variety of purposes. Luminous life belts may be clearly seen at night when thrown into the water; floating buoys so painted can be distinguished at night; sign-posts, etc., etc.

Much more extensive use can be made of some luminous paints which are independent of daylight and can be used in any position, underground in coal mines and on railways. The dials of clocks and watches and scientific instru-

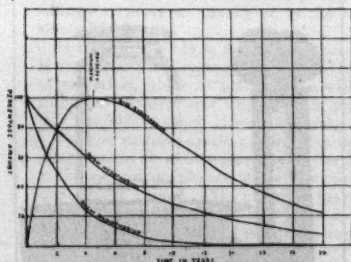


FIG. 8.—MESO-THORIUM CURVES (Rutherford).

ments can thus be made clearly visible. There are many interesting uses in the Army and Navy, but at the present moment it is not desirable to go into details. Star maps painted with radium paint are very realistic and useful for night marching. (Specimen shown.)

Methods of Mixing and using Radium Paint.

—The radium compound can be applied to dials by mixing the powder with a little varnish; for ordinary uses good mastic varnish serves the purpose very well. The best way of mixing is to pour a little heap of the compound in the form of powder into the middle of a watch glass,

then add a few drops of turpentine to the base of the heap, so that the turpentine is drawn up by capillarity, thus driving out the air. About the same quantity of varnish as turpentine may now be added, the whole being thoroughly mixed with the brush which is going to be used. On no account must the mixture be made by using a pallet knife. This would greatly reduce the luminosity by damaging the crystals of sulphide of zinc; each crystal should be preserved intact for full luminosity.

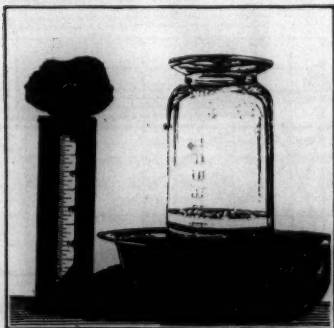


FIG. 9.—SHOWING THE VOLUME OF HELIUM OBTAINABLE FROM THE MINERAL FERGUSSONITE, SIZE SHOWN.

The mixed paint is most readily applied by using a fine sable brush giving successive coats, allowing each to dry thoroughly before putting on the next. The minimum possible quantity of varnish should be used. The mere addition of varnish cuts down the luminosity of the paint to about $1/4$ th or $1/5$ th proportional to the amount of varnish. Celluloid, crystal, or copal

varnishes may also be used. If the paint is not protected by a glass cover a final coat of varnish only should be given as a protection. Where the paint is to be applied directly to silver, iron, brass or copper, a preliminary coating of zinc white mixed with varnish should be given. White lead must not be used for this purpose, as it must be remembered that the zinc sulphide will re-act with the same, producing a dark sulphide, which will cut down the luminosity. Further research is desirable in connection with these luminous compounds, which is a matter of national importance. It is desirable that a substance should be found which does not suffer fatigue or decay so readily as in the case of zinc sulphide.

It may be possible to find a substance which is capable of being regenerated by another form of radiation, which is not the case with zinc sulphide. Experiments with crystals point to this possibility, which is very desirable.

Painted surfaces should not be submitted for measurement for at least seven days after drying of the final coat which may contain the minimum quantity of varnish. The necessary manipulations of the compound in powder and liquid form permits the escape of emanation and time is required for recuperation.

The maximum luminosity can only be obtained by a thickness of 1 gramme of compound to 4 or 5 superficial square inches.

Discussion.

THE PRESIDENT (MR. F. J. CHESHIRE) said it was not usual for the President of the Society to interpose at that stage of the proceedings, but one very interesting problem had been suggested by Mr. Glew upon which it was rather important to have the accumulated knowledge of all present. That problem he would call the "keyhole problem" (Mr. Glew had jokingly suggested luminous keyholes for late diners

out), and it was this: Suppose that two similar small glass tubes are taken, the first containing a mixture of a gramme of zinc sulphide and 0.4 mg. of radium bromide, and the second containing a mixture of one gramme of zinc sulphide and a mass of mesothorium equivalent to 0.4 mg. of radium bromide. The two tubes to be then placed in a dark room subjected to a photometric examination at intervals during a period of two years say, and the time candle power curve for each of the tubes thus determined. Can Mr. Glew draw for us, even approximately, these two curves upon the blackboard? They could see how important it was that if one wanted to efficiently indicate the position of a keyhole that this problem should be solved. If one composition was much brighter than the other and happened to be cheaper, naturally, of course, it would be used, so that it would be convenient to consider the problem from the keyhole point of view.

MR. F. HARRISON GLEW asked whether there was supposed to be anything else in the tubes besides the radium and the mesothorium.

THE PRESIDENT replied in the negative; he said he was taking a purely commercial product consisting in the one case of zinc sulphide and radium, and in the other case of zinc sulphide and mesothorium.

MR. F. HARRISON GLEW said the point had been gone into to some extent in the paper, but the full data were not available to enable the problem to be dealt with in its entirety. If it were possible to get mesothorium of absolute purity and radium of absolute purity it would be a different matter. Of course, all theoretical calculations were based on the assumption that the substances in question were absolutely molecularly pure, but actually pure mesothorium had not been prepared yet, and what was obtainable in the market as "pure" mesothorium was more active both in Alpha rays

and Gamma rays than an equal weight of radium, and was consequently more luminous. If absolutely pure, it would in fact be more than 100 times as luminous as radium, taking molecular weight for molecular weight. But the mesothorium sold in the market was impure, and was bought on its Gamma ray activity. While its Alpha ray activity was more than 100 times that of radium, its Gamma ray activity was also enormously greater than that of radium, considered molecularly and not as it was bought on the market. As purchased on the market for the purpose of making paint, it was bought on its Gamma ray activity, but it was for its Alpha ray activity that it was really required for the purpose of paint-making. The difference came in commercially in the matter of cost, and it was possible to make with mesothorium a 70 per cent. brighter paint than with radium for the same amount of money. Of course, it all depended upon the age of the mesothorium. The probability would be—and especially just now that when bought it was at its peak, and not fresh-made. There was no fresh mesothorium being made now, and in four years' time it bounded up to double its original activity. If bought when new, it would have only cost half its present price, and it was impossible to test its age either by means of the electroscope or the Alpha rays. With every bit of mesothorium bought one ought to have a pedigree. (Laughter.) The President's "keyhole problem" opened up a very important and practical question, and he was sorry he could not answer it in its entirety because the data had not been fully worked out. He might, however, say this—that with new mesothorium it was possible to get about 70 per cent. more brilliancy for the same Gamma ray activity.

THE PRESIDENT thought it was clear from what Mr. Glew had said that if one were taking a house on a three years' agreement the key-

hole should be painted with "pedigree" mesothorium. (Laughter.)

PROFESSOR SILVANUS THOMPSON said that the feeling uppermost in his mind would be best expressed, if he might be permitted to do so, by moving a vote of thanks to Mr. Glew. They had been shown a number of experiments and a number of entirely novel things, for which they could not do less than thank Mr. Glew, and he was afraid, as far as he was concerned, he could not do much more. He might perhaps throw out one thought. Two thousand years hence very little of the radium now in existence would remain, and what remained would have gone down in value to the extent of one-half. But there would be more radium coming into existence; it was always coming into existence by the degeneration of uranium. These various substances were each formed by the degeneration of other and more primary substances, and the ultimate source of them all, as far as they yet knew, was uranium. This naturally gave rise to the speculation as to what would happen in the event of there being no further supply of uranium; there would then be nothing out of which radium could be produced. Well, but where did the uranium come from which we possessed now? They were told it had been degenerating for the last thousand million years or so into radium, and one would have been inclined to suppose it must have long since all vanished and become converted into lead, or something of that sort—unless, indeed, it was continually being manufactured, or produced in some way. He took it, therefore, as self-evident that uranium must be coming freshly into existence to-day; either that, or else we were drawing on our limited store, and none would be left a thousand million years hence. (Laughter.) Now how did that uranium come into existence? Some physical process certainly must have brought it into existence originally, even if none had been made for a million years

or more. There must be some physical process going on in the crucible of the earth's crust at some stage or other of which radium, or the parent of radium, uranium, was being manufactured, and it ought to be possible to discover what that process is. It was a profound mystery at present, but it ought to be possible to reverse the process, and to reconstruct from uranium whatever it was that uranium originally came from. He had asked himself, sixteen or seventeen years ago, when these things were first discovered, whether it was not possible to find some process by which it might be possible to reconstruct these materials of higher activity out of those of lower potency. But like everybody else who had tried it, he had failed lamentably. He had hoped to be able to pound some activity into these lower substances by bombarding them by means of α -rays, or by applying heat under some extraordinary conditions, or by alternate, sudden heatings and coolings. But no; no process he or anyone else had yet been able to apply had been able to invert the order of degeneration. But it was not impossible. He did not believe in any such thing as finality in research, and if this great discovery was ever going to be made, then our nation should make up its mind to be first in the field.

MAJOR WILLIAMS, in seconding the vote of thanks, said he could not add anything to the discussion; he had merely come for the purpose of enjoying the lecture, and he had not been disappointed.

THE PRESIDENT said he could not help saying, before putting the vote of thanks, that during the delivery of the lecture it had been strongly present to his mind that the lecture was being given by one who had made the subject a life-long hobby—(hear, hear) and this, as they all knew, was the secret of success. A few years ago no one would ever have suspected that a hobby of that character would be capable of assuming national importance at such a time

as the country was now passing through. But they might take it from him that this particular hobby had assumed very great national importance indeed, and that the country was at present very much indebted to Mr. Glew for the splendid services which he was placing at the nation's disposal in this matter. (Applause.) While listening to the lecture he had been reminded of a story he had heard some years ago. A Senior Wrangler fresh from his triumphs went down to the country to spend a holiday, and there he met a certain vicar who had also been high on the Wrangler List when a young man. They went out for a walk, and saw a rainbow. The vicar looked at it, and said to his companion: "Ah, look at that beautiful rainbow! When I was at College, I knew all about that rainbow. I could trace all its rays through all their various angles, and tell you all about it; but now, all that I know about it is that it has got something to do with *theta*!" (Laughter.) He was perfectly certain that the time would never come in Mr. Glew's life when he would be able to pick up a piece of radium and say: "Ah, at one time I could have told you all about this, but now all I know of it is that it has something to do with *alpha*." (Renewed laughter.)

The vote of thanks was carried by acclamation, and Mr. Harrison Glew having briefly acknowledged the compliment, the proceedings terminated.
